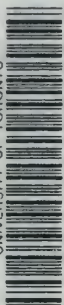


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A  
COMPLETE TREATISE  
ON  
ELECTRICITY,  
IN  
THEORY AND PRACTICE;  
WITH  
ORIGINAL EXPERIMENTS.

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By TIBERIUS CAVALLO, F.R.S.

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IN THREE VOLUMES.

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VOLUME III.

*Containing the* DISCOVERIES *and* IMPROVEMENTS  
MADE SINCE THE THIRD EDITION.

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LONDON:  
PRINTED FOR C. DILLY, IN THE POULTRY.

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T H E

P R E F A C E.

**T**HE rapid advances made in the Science of ELECTRICITY since the first publication of this Treatise, have furnished considerable additions for every subsequent edition. Those additions having been interspersed throughout the Work in the three first editions, have obliged several persons, who were desirous of being informed of the new improvements, to purchase the Work more than once. In order to avoid this inconvenience,

the present edition has been published in *three volumes*; the first and second of which have been reprinted without any material alteration, whilst the new materials are contained in this *additional* volume, which, independent of its being sold with the other two, may be purchased by itself, to complete the third edition.

Besides a great many other articles, the Reader will find in this volume an account of the new subject of ANIMAL ELECTRICITY, which may be justly considered as one of the greatest discoveries made in the present century.

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*N. B.* When the whole of this volume was printed, I had an opportunity of perusing Mr. SAUSSURE's Experiments and Observations on the Atmospheric Electricity, and on the Electricity which is produced by the evaporation of water, &c. I shall therefore just mention, that this ingenious philosopher, having made a variety of well-conducted experiments on evaporation, found, that positive Electricity was produced by the evaporation of water from some other heated substances, besides those mentioned in N° V. page 270 of this Volume. Positive Electricity was produced when he dropped a lump of red-hot iron into a small quantity of water; but when water was thrown at successive intervals into a large iron crucible highly heated, then no Electricity appeared at first; some time after a positive Electricity was manifested, and at last the Electricity became negative. Copper did likewise produce the two Electricities; but silver, and porcelain produced the negative Electricity.

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AN ACCOUNT  
OF THE  
*Discoveries concerning Muscular Motion*  
WHICH HAVE BEEN LATELY MADE,  
AND ARE COMMONLY KNOWN UNDER THE NAME OF  
ANIMAL ELECTRICITY.

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THE subject of muscular motion, which has exercised the genius of philosophers from time immemorial, is one of those arcana of nature, that have hitherto eluded the investigation of human industry. Innumerable experiments have been made in vain, and the labours of one man have hardly ever produced any thing more than the refutation of another man's hypothesis.

This labyrinth of obscurity has at last received a ray of light from the recent discoveries of Dr. Galvani of Bologna. A new way has been opened to promising

VOL. III.

B

expe-

experimental investigation, and many ingenious persons throughout Europe are now pursuing the track with care and assiduity.

The original discoveries were announced to the world in a quarto book, consisting of fifty-eight pages with four large plates, and entitled, *Aloysii Galvani de Viribus Electricitatis in motu musculari Commentarius. Bononiæ 1791.* Abstracts of this work were soon after inserted in various periodical publications, and these have been followed by farther discoveries made by other ingenious persons, and especially by Professor Volta, Dr. Eusebius Valli, Dr. Monro, and Dr. Fowler. The scattered materials are numerous for want of a theory; but neither can a theory be formed, nor even farther investigation be instituted, without a comprehensive view of all that has been done concerning the present experiments.

Having myself made several experiments on this interesting subject, mostly in conjunction with Dr. James Lind, F. R. S. I have in a manner been under the necessity

ceffity of collecting and methodifing not only the facts that have appeared in print, but thofe alfo which have been communicated to me through letters, or by means of verbal information; I think it neceffary to add this account to my *Treatife on Eleâtricity*, and hope that a compendious view of the fubject may be of ufe to thofe perfons, who are difpofed to engage in fo promifing a field of curious and ufe-ful in-veftigation.

It is for the fake of brevity, and as this account is not intended to be hiftorical, that I fhall not follow the order of time, nor particularly point out the perfon to whom each difcovery or obfervation belongs. Their names will undoubtedly be recorded, with praife and gratitude, in many fubfequent publications.

Previous to thefe difcoveries, the name of *Animal Eleâtricity* was given to the analogous powers of three fifhes; namely, the *Torpedo*, the *Gymnotus Eleâtricus*, and the *Silurus Eleâtricus*.—Whenever a communication is made by means of fubftances that

are Conductors of electricity, between one part and another of those fishes, and a man or other animal is placed in the circuit, which forms the said communication; the man or other animal feels a shock analogous to that occasioned by the discharge of a Leyden phial. If the circuit of communication be interrupted, or formed of substances which are not Conductors of electricity, the shock is not felt, or at least a very slight tremor is perceived in the hand that touches the animal. The gymnotus alone affords a luminous spark, which becomes evident by making an exceedingly small interruption in the circuit of communication. Excepting these properties, it is not known that they have any thing in common with artificial electricity; and every property vanishes with the entire death of the animal\*.

The

\* A fourth electrical fish was found on the coast of Johanna, one of the Comoro islands, in lat.  $12^{\circ} 13'$  South, by Lieutenant WM. PATERSON, and an imperfect account of it was published in the 76th Vol. of the Phil. Trans.

“ The fish is described to be seven inches long,  $2\frac{1}{2}$  inches



The discoveries of Dr. Galvani were made principally with frogs, and after the animals were dead. He first discovered, that a frog dead and skinned, is capable of having its muscles brought into action by means of artificial or atmospherical electricity. And, secondly, he discovered, that independent of any artificial or natural Electricity, the same motions may be produced in the dead animal, or even in a detached limb, by merely making a communication between the nerves and the muscles, with substances that are Conductors of electricity. If the circuit of

“ inches broad, has a long projecting mouth, and seems  
 “ of the genus *Tetrodon*. The back of the fish is  
 “ a dark brown colour, the belly part of sea-green,  
 “ the sides yellow, and the fins and tail of a sandy  
 “ green. The body is interspersed with red, green, and  
 “ white spots, the white ones particularly bright; the  
 “ eyes large, the iris red, its outer edge tinged with  
 “ yellow.”

Whilst this fish is living, strong shocks, like electrical shocks, are felt by a person who attempts to hold it between his hands. Three persons are mentioned in the account to have experienced this property of one of those fishes; but the want of opportunity prevented the trial of farther experiments.

## 6      *A* COMPLETE TREATISE

communication be made by means of Non-conductors of electricity, as glass, sealing wax, and the like; no motion will take place.—It is owing to these circumstances, that the name of Animal Electricity has been bestowed on this wonderful property of organized animal matter.—These properties have been verified in a great many other animals besides frogs.

Previous to Galvani's discoveries, I find only one curious fact recorded, which seems to be materially connected with the present subject. It is related in a letter of Dr. Cotugno, Professor of Anatomy at Naples, to the Chevalier Vivenzio of the same place. This letter, a translation of which I shall subjoin, was published in the Italian translation of a book of mine on Electricity, by the above-mentioned Chevalier Vivenzio.

“ SIR,

“ THE observation, which I mentioned some days ago, when we were discoursing together of the electrical animals, upon which I said that I believed the mouse to be one of the number, is the following :”

“ Towards



“ Towards the latter end of March I was sitting with a table before me; and observing something to move about my foot, which drew my attention, looking towards the floor, I saw a small domestic mouse, which, as its coat indicated, must have been very young. As the little animal could not move very quick, I easily laid hold of it by the skin of the back, and turned it upside down; then with a small knife that laid by me, I intended to dissect it. When I first made the incision into the epigastric region, the mouse was situated between the thumb and first finger of my left hand, and its tail was got between the two last fingers. I had hardly cut through part of the skin of that region, when the mouse vibrated its tail between the fingers, and was so violently agitated against the third finger, that to my great astonishment I felt a shock through my left arm as far as the neck, attended with an internal tremor, a painful sensation in the muscles of the arm, and with such giddiness of the head, that being affrighted, I dropped the mouse. The stupor of the arm lasted upwards of a quarter of an hour,

nor could I afterwards think of the accident without emotion. I had no idea that such an animal was electrical; but in this I had the positive proof of experience."

*Naples, Oct. 2d. 1784.*

1. The action of Electricity on a dead and skinned frog (and indeed on other animals more or less) occasions a tremulous motion of the muscles, and generally an extension of the limbs.

2. When the nerves of a frog recently killed and deprived of its integuments, are exposed to an electrified atmosphere, or, in short, are so disposed, as that by the action of an electrical machine, or of any electrified body, a quantity of electric fluid is caused to pass through them, a contraction of the muscles takes place, with a tremulous convulsive motion, which may be reiterated for some hours after. Dr. Galvani prepared a frog, having its legs attached to a part of the spine, but separated from all the rest of the body; and observed that whenever a spark was taken from a large prime Conductor of an electrical machine situated at  
some

some distance from the prepared part of the animal, those legs moved with a kind of spasmodic contraction, sometimes strong enough to jump a considerable way. It was found necessary to place the prepared legs contiguous to some good Conductor not insulated.

3. Whether the frog be brought into actual contact with the electrified body, or not; whether it be made to receive the spark itself or not, the motions happen equally well, provided a quantity of electric fluid be caused to pass through it, which may be done merely by the pressure or action of electric atmospheres.—When the electricity is made to pass through the frog, by the immediate contact of the electrified body, a much smaller quantity of it is sufficient to occasion the movements, than when it is made to pass from one Conductor to another at a certain distance from the prepared animal.

4. If the electric atmosphere be so strong as to occasion little sparks between the Conductors contiguous to the animal, or if it  
be

be capable of affecting an electrometer placed near the animal; then even a whole frog, a lizard, a mouse, or a sparrow, will be strongly affected with violent convulsions. When the animal is insulated, and the electricity is made to pass through its body, then a whole living frog is affected by the passage of so small a quantity of electricity as is discharged by a middling prime Conductor, that is just capable of affording a small spark. In this case, if a Leyden phial be used, a much smaller quantity of electricity will be found sufficient for the purpose, viz. such a charge of it as cannot afford a spark, but that can just produce a sensible divergence of the pendulums of an electrometer.

5. But a frog, prepared, especially after the manner of Dr. Galvani, is affected by an incomparably smaller quantity of electricity. Mr. Volta has observed, that so small a quantity of electricity as is absolutely incapable of occasioning a divergence in the most sensible electrometer, but such as may be observed by his condenser of electricity,  
is

is sufficient for the purpose \*. Thus if a Leyden phial be charged, and discharged, and afterwards be disposed, so that the prepared frog be placed in the circuit between its inside and outside coating; the passage of that small residuum is fully sufficient to produce the contractions, &c.—By being sensible of so small a quantity of electricity, the prepared frog becomes a most sensible sort of electrometer, which perhaps hereafter may be of singular use in some nice electrical experiments.

6. When the preparation and disposition of the frog is such, as that the electric fluid

\* Mr. Volta estimates the quantity of electricity, which can be perceived by means of his condenser, to be the 500<sup>th</sup> or 600<sup>th</sup> part of that quantity, which is necessary for affecting my electrometer. Upon this I must observe, not for the sake of comparing the condenser with the electrometer, but merely to convey a just idea of the quantity of electricity necessary for the present purpose; that Mr. Volta's condenser cannot render sensible a quantity of electricity absolutely small, but collects together, and condenses a certain quantity of it, which, by being diffused through a considerable space, exists in so rare, or uncondensed a state, as not to be capable of affecting an electrometer.

must pass through a nerve to the muscle or muscles, then the movements are in general much stronger, than when it is applied to any other part of the body.

7. Dr. Galvani had the curiosity of trying whether the electricity of the clouds produced the same effect on the prepared limbs, as the artificial electricity of the ordinary machines, and for that purpose he extended a Conductor from the top of a house to the prepared animal, which was sometimes laid on a table in the open air, and at other times was enclosed in a glass receiver. On this preparation the thunder and lightning produced the same effects as the sparks from the electrical machine. The same contractions took place, and they were stronger or weaker according to the distance and quantity of lightning. Thus far the effects might have been naturally expected; but a remarkable circumstance was observed, which serves to explain another phenomenon of nature. — It was found, that instead of one contraction at every clap of thunder, the limbs were affected with a sort of tremor or succession of convulsions,



vulsions, which seemed to be nearly equal in number to the repetition of the thunder, viz. that succession of explosions which forms the rumbling noise of thunder\*. Now this observation proves, that the rumbling noise is not the echo of a single explosion, or the successive arrival of the vibrations produced at different distances, though at the same moment of time; but that it is produced by a quick succession of several explosions, which indeed seems to be confirmed by observing, that the clouds are very imperfect Conductors, in which state they are not likely to receive a full and single stroke of electricity from other clouds, or from the earth.

8. The sensibility of the prepared animal is greatest at first, but it diminishes by degrees till it vanishes intirely. In general, frogs, and other animals with cold blood, retain the property of being affected by electricity much longer than those possessed of hot blood. With some of the latter, the sensibility is very weak, and will hardly

\* Galvani de viribus electricitatis in motu musculari, p. 15.

last for a few minutes after the death of the animal; whereas some of the animals with cold blood, and especially frogs, which are by far the fittest animals for such experiments, have frequently retained that property for upwards of 12 hours, and sometimes even for two or three days.

9. Thus far we have described the effects of electricity on dead animals; we shall now come to the most curious part of the subject, which is, that the same motions, the same convulsions, &c. and for a time about equally long, can be produced in dead, and even in living animals, without the aid of any apparent electricity.—In an animal recently dead, detach a nerve from the surrounding parts; taking care to cut it not too near its insertion into the muscles; remove the integuments from over the muscles depending on that nerve; take a piece of metal, as a wire, touch the nerve with one extremity, and the muscles with the other extremity of the wire, and the consequence will be, that the muscles will move exactly as if a quantity of electricity were sent through them. This experiment  
will



will answer equally well when the preparation is laid upon an insulated stand, as when it communicates with the ground. If the communication between the nerve and the muscles, instead of being formed by means of metal or other Conductor of electricity, be formed of substances that are non-conductors, as glass, sealing wax, oils, &c. then no motion will take place.

10. When the application of the metal or metals is continued steadily on the part, the contractions will cease after a certain time, and on removing the metal, seldom, if ever, any contraction is observed.

11. Our ignorance of any other power in nature excepting electricity, that can be transmitted with great rapidity through metals, water, &c. and that cannot be transmitted through glass, resins, and certain other substances, induces one to imagine, that the above-mentioned effects are owing to electricity, generated in some part or other of the animal or of the surrounding bodies, as the metals or other bodies which form the communication between the nerves  
and

and muscles \*. It is not easy to conceive how a quantity of electric fluid can be generated or accumulated in one part, and a deficiency of it can take place in another part, of the body of an animal, which is in every part a Conductor of electricity; and even if it consisted of Conductors, and of Non-conductors combined together, it would be difficult to account for that quick production of electricity, which the effects observed in these experiments seem to require.

12. However, notwithstanding the obscurity in which the subject is at present involved, I must take the liberty of mentioning a consideration which occurred to me when I first heard of these discoveries, and which may probably be of use to others in the investigation of so wonderful a phenomenon.—Electricians have divided the

\* Heat is more easily conducted by certain bodies than by others, and those bodies which transmit it more easily are in general Conductors of electricity also; but the slowness with which heat is propagated, besides other obvious considerations, seem to exclude it from the supposition of its being essentially concerned in the present experiments.

various

various substances of our globe into Conductors and Non-conductors of electricity. The great difference between their peculiar properties is certainly evident and useful; since, without such a difference, we could not have had any knowledge of the existence of electricity. But, strictly speaking, there is no substance in nature, that may be called a perfect Conductor, or a perfect Non-conductor of electricity. The various states of the same substance, with respect to heat and cold, or of rarity and consistency, are attended with a considerable variation of its conducting or non-conducting property. Glass itself becomes a Conductor by being heated to a certain degree, and any one of the metals opposes a certain resistance to the passage of the electric fluid \*. But omitting those bodies which are the most perfect of their class, let us consider a case in which some electricity is produced and dissipated amongst substances that are less perfect.—When a person with a dry hand rubs a piece of paper sufficiently

\* For proofs of this assertion, see the first volume of this Treatise.

dry, some electricity will be produced or accumulated upon the paper. If the paper thus electrified be held by one corner, the electricity will presently disappear. This effect is easily explained, by considering the imperfect conducting and non-conducting state of the bodies concerned. The friction of the hand accumulates some electricity upon the paper; but the paper being an imperfect Conductor, cannot carry away to the other hand that holds it, all the electricity as quickly as it is accumulated upon it; the friction being interrupted, the electricity is gradually dissipated. When the paper is less dry, the electricity will remain for a shorter time, or it may be conducted away as quickly as it is generated. Now, in order to apply this effect to the case of animal electricity, we must first consider, that though the whole animal is a Conductor of electricity, yet every part of it is not an equally good Conductor. Admitting then, or supposing, that in the body of the animal a quantity of electricity is produced by some cause or other to us unknown, it must follow, that this electricity must be conducted by, or expanded through, some parts

parts much easier than through others; consequently the former will contain more of that power, than the latter. The metal, then, which is applied to form the communication between the former and the latter, being a better Conductor than either, restores the equilibrium, and thus the animal electricity itself may produce the effects of the artificial electricity.—This consideration removes one difficulty, viz. it shews that an unbalanced state of electricity may take place, and that the equilibrium may be restored in a body, or congeries of substances, which are all Conductors, though not equally good Conductors of electricity.

13. The conducting communication between the muscle and the nerve may consist of one or more pieces, and of the same or of different bodies connected together, as metals, water, a number of persons, and even wood, the floor, and walls of a room. But it must be observed, that the less perfect Conductors will answer only at first, when the prepared animal is vigorous; but when the power begins to diminish, then only the more perfect Conductors, as the

metals, will answer, and even these are attended with various effects.

14. It is in this nearly exhausted state of the animal electricity, that the various conducting powers of different substances can be observed; and thus amongst the experiments which I made with Dr. LIND, we formed the following list of Conductors, which are arranged in the order of their perfection, beginning with the best. Yet I do not mean to offer this as a very correct arrangement; for though it has been deduced from a great number of experiments, their result has not however been very constant. A considerable difference is frequently occasioned by circumstances that are hardly perceivable; such as the changeable state of the prepared animal, the surface of the substances used, the quantity of contact, &c.

Malleable platina.

Silver.

Gold.

Quicksilver.

Copper.

Brass.



Brass.

Tin.

Lead.

Iron.

The human body.

Salt water.

Fresh water.

15. The metallic ores are not so good Conductors as the purified metals themselves, and their conducting power is various according to the nature of the ores, but even the metallic salts are tolerably good Conductors.

16. It is very remarkable, that the flame of a tallow-candle, which is a good Conductor of common electricity, will not conduct the animal electricity, when placed in a short interruption made in the circuit of communication. Charcoal placed in the same situation as the flame of the candle, was also found to be a Non-conductor, except when it was actually burning, in which state it conducted tolerably well; but Mr. VOLTA says, that he has found some pieces of charcoal that acted as well as the metals.

—Dr. VALLI observed, that human bodies are not all equally good Conductors. Out of four persons in a company, he found that when two of them formed the circuit of communication between the nerve and the muscles of a frog, the motions took place very well. When a third person formed the circuit, the motions were very weak; but that when the fourth person formed the communication, no motion took place. This experiment, he adds, was often repeated with the same success. It may however be observed, that the greater or less dryness of the skin of the hands in those persons, might have been sufficient to produce the observed diversity of effect.

17. Vitriolic acid, and, what is very remarkable, alcohol, appear to conduct this property rather better than water.

18. This power, like the common electricity, passes through the substance, and not only over the surface of Conductors; hence a wire surrounded with sealing-wax, or other non-conducting substance, except where it touches the animal preparation, will



will answer as well as when it is not coated with a non-conducting substance.

19. When various Conductors are placed contiguous to each other in the circuit between the muscle and the nerve, their contact must be perfect; otherwise the desired effect will not take place. To lay one metal upon the other is seldom sufficient, unless they are pressed against each other. If two or more persons join hands, the contact must be frequently rendered more perfect by the interposition of water, viz. by moistening the fingers, especially with salt water. Sometimes when the fingers are greasy, or have much perspired, the animal electricity will not be communicated through them. In this case, it becomes necessary to wash them well, and then to dip them in salt water. An interruption, that could not exceed the 200<sup>th</sup> part of an inch, which I made in the metallic circuit of communication, did absolutely prevent the communication of the power between the nerves and muscles of six frogs, which had been prepared and connected all together.

20. The arteries and the veins are not so good Conductors as the nerves; for when a blood vessel forms part of the circuit of communication, the contractions will take place only when nervous ramifications are adhering to it, and if these be carefully separated, the motions will not happen.—The same thing may be said of the tendons, the bones, and the membranes; for when either of those parts is separated from the body, and is introduced in the circle of communication between the muscles and nerves of a prepared frog, no motion will ensue, excepting indeed when those parts are full of moisture, and are in immediate contact with the nerve of the prepared frog.—Dry nerves are not Conductors of animal electricity. Dr. VALLI found, that the internal substance of a nerve conducts much better than its external, or coat,

21. If part of the nerve be wrapped up in a thin piece of metal, as tin-foil or sheet-lead, and the conducting communication, or conducting metallic rod, be applied from this *coating* or *armour* to the muscles, the motions will, in that case, be much stronger,  
The

The muscle itself may also be armed with, or simply laid upon, metal, and on completing the communication between the armour of the nerve and that of the muscle, or muscles, the motions will be very vigorous, and will continue much longer, than when no coating or armour is used. Besides metals, the armour may consist of water or of other Conductors, as will be shewn in the sequel. The use of this armour seems to be the augmentation of the points of contact. The effect has been observed to be much greater, and to succeed more constantly, when the conducting rod is put first in contact with the muscle or its coating, and is then brought with its other extremity into contact with the armour of the nerve, than when it is placed in contact with the nerve first. Hence, when the power of the animal is much weakened, the former way will answer, but the latter will not.

22. It is very singular that in this experiment it is necessary to employ two different metals, viz. One to be in contact with the nerve, and another to be in contact

tact with the muscles ; for if they be of the same sort, as both of silver, or both of tin-foil, the contractions will not take place. It must however be observed, that in the beginning, when the power of the prepared animal is strong, the convulsions will happen even when the two coatings are of the same sort of metal ; though not nearly so well as when two metals are used. But indeed in the beginning, when the animal electricity is strong, the motions are frequently produced without any coatings, and even without any conducting rod. The mere striking of the table, or the approach of a piece of metal, without any actual contact, will frequently excite the movements\*. But this strong sensibility is of short duration ; after which period the two armours of the same sort of metal will not occasion any motion whatever.—The least difference, however, in the quality of the two coatings, is sufficient to produce weak motions, as when they are of silver of different degrees

\* “ I have likewise, *says Dr. VALLI*, seen in two frogs “ the movements occur at the distance of half an inch “ from the scissars, and which ceased at the moment I “ insulated the scissars.” Exp. on An. El. p. 40.

of purity, or of different sorts of lead, &c.

—It is for the same reason, that if they consist of two metals that have a great affinity to each other, the effect is not so great as when the two metals are more dissimilar in their nature. Thus it has been found, that gold and silver do not answer so well as silver and zinc, or gold and lead. Either gold or silver, or steel, or copper, or molybdena, when combined with tin or lead, or especially with zinc, are very good exciters of the contractions in prepared animals. But the combinations of any two of the former metals are much inferior in power\*.

—Large pieces of the metals, and with ample surfaces, seem to answer better than small and compact pieces for these experiments; for with the former contractions may be excited, when the latter are unable to produce any effect. Farther experiments

\* Dr. VALLI says, that with the prepared fore-legs of a rabbit, he found that the following combinations of metals produced no movements, viz. glaziers lead, and assay lead, lead and iron, lead and gold, lead and copper, lead and zinc, or lead and antimony. Experiments on An. El., p. 45. But it seems that those conclusions were not grounded upon a sufficient number of experiments.

are however required to ascertain all the particulars relative to this part of the subject.

23. The abovementioned singular properties, together with some other facts, which will be mentioned in the sequel, induced Mr. VOLTA to suspect, that possibly in many cases the motions are occasioned by a small quantity of electricity produced by the mere contact of two different metals; though he acknowledges, that he by no means comprehends in what manner this can happen. This suspicion being entertained by so eminent a philosopher as Mr. VOLTA, induced Dr. LIND and myself to attempt some experiments, which might verify it; and with this view we connected together a variety of metallic substances in diverse quantities, and that by means of insulated, or not insulated communications; we used Mr. VOLTA's condenser, and likewise a condenser of a new sort; the electrometer employed was of the most sensible sort; and various other contrivances were used, which it will be needless to describe in this place; but we could never  
 \* obtain



obtain the smallest appearance of electricity from those metallic combinations. Yet we can infer no other conclusion, but that if the mere combination, or contact, of two metals produces any electricity, the quantity of it in our experiments was too small to be manifested by our instruments.

24. It has been lately asserted, that by means of the *doubler of electricity* (an instrument of which particular notice will be taken in the course of this volume) the production of different sorts of electricity, by the mere contact of different metals, has been fully ascertained; and that zinc has been found to communicate positive electricity, whilst silver gives the negative. If this discovery has really been made, we must acknowledge it to be a very considerable improvement in the science of electricity; but, for my part, I must confess that my endeavours to prove it, either by a doubler of electricity made in the most unexceptionable manner, or by other instruments, have as yet proved unsuccessful; nor have I met with any person who could  
shew



shew me so singular a property of metallic substances.

25. The motions will be also excited when the metals are not in immediate contact with the prepared limb, provided they form part of the circuit of communication. Thus lay a prepared limb upon a table, hold the nerve in one hand, and a piece of zinc in the other hand: lay a piece of silver upon the table at about one or two inches distance from the prepared muscles, and make a communication between the muscles and the silver by means of water or some other good Conductor. If now you touch the said piece of silver with the zinc which you hold in one hand, the contractions will take place. The same effect will happen, if the two pieces of metal be first put in contact, and then the operator touches the nerve of the preparation with his finger.

26. The preparation of the frog, or other animal, for this experiment, generally consists in detaching one of the principal nerves from all the surrounding parts, where it enters

ters a member susceptible of motion, and arming it with a metallic foil. On making the communication, the motion will take place; but the preparation which answers best, is delineated in *fig. 1 and 2 of the plate*, which, for the sake of distinction, we shall in the following pages, call *the usual preparation*; it being, in fact, that which has been more frequently and more advantageously used.—It is made in the following manner.—Separate with a pair of scissars the head and upper extremities of a frog, in the line AB, from the rest of the body. Open the integuments and muscles of the abdomen, and remove the entrails; then you will lay bare the crural nerves, as shewn in *fig. 1<sup>st</sup>*. which in this animal come out of the spine at a considerable distance above the pelvis; viz. from the line CD. Then pass one blade of the scissars under the said nerves, and cut off the spine with the flesh close to the thighs in the line EF, by which means the legs will remain attached to the spine by the nerves alone. This done, leave only a small bit of the spine attached to the crural nerves, and cut off all the rest. Thus you will have the two legs GH, *fig 2.*

of a frog adhering to a bit of the spine ACD, by means of the crural nerves CE, DF. These legs must be flayed in order to lay bare the muscles. The metallic armour, which generally consists of a piece of tin-foil, must be placed round the nerves very near the spine, viz. at CD, or round the whole bit of spine AD, and the extremities of the nerves next to it. A frog thus prepared, and touched by means of a conducting rod applied to the muscles and to the armour of the nerves, will act vigorously for a considerable time. Some contractions have been observed several hours, and even days after; but the power is gradually diminishing, and in general it can seldom be perceived after two or three hours.

27. With a frog prepared in the above described manner, one may shew the experiment various ways; but the two following methods are peculiarly eligible, because they produce very strong and striking movements.—Hold the preparation by the extremity of one leg, the other leg hanging down, with the armed bundle of nerves  
and

and bit of spine lying upon it. In this situation interpose a piece of silver, as a half-crown, between the lower thigh and the nerves, so that it may touch the former with one surface, and the metallic coating of the latter with the other surface, or with its edge; and you will find that the hanging leg will vibrate very powerfully, sometimes so far as to strike against the hand, which holds the other leg.

28. The other method is the following: Put two wine-glasses full of water contiguous to each other, but not actually touching. Place the thighs and legs of the prepared frog in the water of one glass; and laying the nerves over the edges of the two glasses, let the bit of spine and armour touch the water of the other glass. This done, if you form the communication between the water of the two glasses by means of the conducting rod, or put the fingers of one hand into the water of the glass that contains the legs; and holding a piece of silver in the other hand, touch the coating of the nerves with it, you will

find that the prepared legs will move sometimes so powerfully as to jump fairly out of the glass. It is however necessary to remark that the effect in this experiment is increased by a sort of artifice, which must necessarily take place; viz. the legs, in order to be placed into the glass, must necessarily be drawn up, and as the effect of the communication, &c. is to produce a sudden extension of the limbs, it must naturally follow that from the resistance of the glass, the preparation must endeavour to jump out of the water.—There is another reason, which contributes to increase the effect in this experiment, which is, that the water, by surrounding the prepared legs, becomes a very ample armour or coating, which touches the legs in every point of their surface.—It frequently happens in this experiment, when the Animal Electricity is strong, that without forming the communication, the motions are excited by only touching the armour of the nerves with a piece of metal. Or if one person touch the coated spine, whilst another person touches the legs, the motions will take

place, though the two persons do not communicate with each other by any thing else besides the floor.

29. In these experiments the effects are exactly the same, whether they be performed on a table in the ordinary way, or in an electrified atmosphere, viz. when the preparation, operator, &c. are all placed on an insulating stool, and are kept electrified by the action of an electrical machine.

30. We have said above, that whenever a limb of an animal is prepared, and the communication is formed between the nerve and the depending muscle, the contractions will take place; but we must now take notice of a very remarkable exception; which is that those parts, the motion of which is subject to the will of the animal, are susceptible of contraction by the application of metals; but of the involuntary muscles, the heart alone is capable of being contracted. This peculiar property of the heart was satisfactorily proved by Dr. FOWLER, with the hearts of frogs, cats, and rabbits. “At length,” *says he,*



“ I was so happy as to succeed completely.  
“ On the 18th of March last, in presence  
“ of my friends, Mr. HUNTER, and Mr.  
“ THOMSON, having dissected away the pericardium from a frog’s heart, which had  
“ an hour before ceased spontaneously to  
“ contract, I removed the muscles, and  
“ cellular membrane covering its nerves,  
“ and large blood-vessels; I then placed one  
“ end of a rod of pure silver in contact  
“ with one side of these nerves and blood-  
“ vessels, and one end of a rod of zinc  
“ on the other, both of them at about the  
“ distance of the third part of an inch  
“ from the auricles of the heart. On  
“ bringing the opposite ends of these rods  
“ in contact with each other, the auricle  
“ first, and then the ventricle of the heart,  
“ immediately contracted, and repeated their  
“ contractions as often as the ends of the  
“ metal rods were made to touch each  
“ other.”——“ The contractions were  
“ both more vigorous, and more constant,  
“ when the metals were placed in contact  
“ with the heart itself, than when touching  
“ only its blood-vessels and nerves.”

“ In



“ In order to the complete success of this  
 “ experiment, it is necessary that the spon-  
 “ taneous contractions of the heart should  
 “ nearly, if not altogether, have ceased;  
 “ and when in this state, the experiment  
 “ is rendered still more satisfactory by re-  
 “ moving the heart from the body of the  
 “ frog, and laying it upon a plate of  
 “ zinc\*.”

31. It has been observed, by way of as-  
 sisting the investigation of the above re-  
 marked singularity, that the muscles which  
 are not subject to the will, do not possess  
 so large and so many nerves as the other  
 muscles.

32. The application of the metallic rod  
 to the prepared nerve and depending limb,  
 does not produce contractions in that limb  
 only; but it contracts several other parts  
 that are left attached to it. Thus, if the  
 crural nerve be detached and armed with  
 metal, whilst every other part of the ani-  
 mal is left untouched, on applying the me-

\* FOWLER on An. Elec. p. 75, 76, and 77.

tallic rod to the said nerve, and to the muscles of the leg, the upper as well as lower limbs will contract: even the eye-lids, and other parts of the head, will be seen to move\*.

33. Dr. MONRO observes, that the application of metals to the head of a frog, or to any part of its spine above the sixth vertebra, does not occasion any convulsions of its hind legs; by which he is led to suppose that the nerves of the hind legs are not derived solely or chiefly from the brain or *cerebellum*.

34. By repeatedly applying the conducting rod to a prepared animal, its power is exhausted much sooner, than if it be used more sparingly. It is very remarkable, that when a prepared frog is almost exhausted of its Animal Electricity by the often repeated application of the metals, its power will be in a great measure restored by leaving it at rest for some time. This is analogous to the recovery of strength,

\* GALVANI de viribus El. in m. m. p. 28.

which

which rest alone can produce in living animals, when over-fatigued, and may probably depend upon the same cause.

35. When a frog, prepared in the usual manner, is almost exhausted of its power by the often repeated application of the conducting rod, remove the armour to another part of the same nerve, especially if it be nearer to the muscles, and you will find the power in a great measure restored. This is a curious observation, and naturally leads us to inquire, what does the nerve lose by the application of the armour in this experiment?—It looks as if that part of the nerve were alone concerned in the production of the power called Animal Electricity.

36. A ligature made on the nerve close to its insertion into the muscle, frequently prevents the motions; but if it be made at a distance from the muscle, the experiment succeeds as well as without the ligature.

37. Dr. VALLI says that the ligature of the nerve opposes the same obstacle to Artificial,

tificial, as it does to Animal Electricity; for when the ligature was in contact with the muscles a very great quantity of electricity was found necessary to produce the movements, in comparison with that quantity of it, which was sufficient to produce the effect, when the ligature was at some distance from the muscle.—I should explain this effect in the following manner: By the ligature the nerve is rendered a more imperfect conductor, because the continuation of its medullary part is, in a great measure, if not intirely, interrupted. Now when the ligature of the nerve is in contact with the muscle, the electricity which is sent through the nerve will run along its substance as far as the ligature; but meeting in that place with the muscle, which is a better conductor than the tied part of the nerve, it will run over the surface of the muscle to the other extremity of the conducting rod, and of course it will not produce any movements, or very slight ones; whereas, when the ligature is at some distance from the muscles, though the tied part of the nerve is not so good a conductor as the rest of it, yet the electricity,

tricity, finding no better conductor at hand, must necessarily pass through it, and then, being conducted through the ramifications of the nerve, is communicated to all the internal parts of the muscles, and hence it produces much more powerful contractions.

38. The Animal Electricity is much more easily weakened by obstructing the circulation of the blood, than by interrupting the nervous communication. Thus, if the sciatic nerve of a living frog be divided, and the crural artery of another living frog, or the other leg of the same frog, be tied fast, so as to stop the circulation through it, and if, some hours or days after this, the legs be prepared in the above described manner, the contractions excited by the metals in that leg, whose artery has been tied, will be found to be weaker and to vanish much sooner than in the other leg, the nerve of which had been divided. Dr. FOWLER, to whom we are indebted for this curious observation, in order to prove as a general proposition, “ that the sanguiferous system contributes more immediately

“mediately than the brain to the support  
“of that condition of muscles and of  
“nerves, upon which the phenomena of  
“contraction depend,” made several well-contrived experiments, for which the preparations generally consisted in evacuating the blood as much as possible both from living, and from recently dead, frogs; and then comparing them with other frogs similarly prepared in every other respect, except that they were not deprived of their blood. The effect was, that by the application of metals, or other stimulus, the former shewed weaker contractions, and ceased to contract much sooner, than the latter.

39. Amongst the other experiments I made with Dr. LIND, we had the curiosity of trying whether the communication of Animal Electricity might be interrupted by surrounding that part of the nerve which is between the armour and the leg with white wax; but on applying the conducting rod, the effect was found to be the same as when the nerve was not enveloped in wax.

40. It



40. It has been said, that it is not absolutely necessary to form a communication between the nerve and the muscle in order to produce the movements; it being sufficient to form the communication between two parts of the same nerve, or between two muscles, or even between two parts of the same muscle; one of those parts at least being armed with metal. The effect, however, is much greater when the communication is made between nerve and muscle. Mr. VOLTA performs the experiment in the following manner: He fastens to a pair of pincers the crural nerve of a dog or lamb, a little above its insertion into the leg; the other extremity of the nerve, being separated from the body, is kept suspended by a thread, or is rested on sealing-wax. At about one eighth part of an inch above the pincers he lays a piece of money or other metallic plate upon the same nerve; then, on passing an exceedingly weak charge of the Leyden phial through that part of the nerve, viz. from the pincers to the other metal, or, without any Artificial Electricity, on completing the communication between the two metals,

a con-



a convulsion and agitation of the whole leg will take place; though in this case the leg is out of the circuit. But it has been observed by Dr. FOWLER, that when the nerve has been wiped dry, or is suffered to remain suspended 'till its moisture is evaporated, this experiment will not succeed; it seems therefore that the moisture, which surrounds the nerve, may form the communication to and from the muscle; and of course that a communication from nerve to muscle seems to be absolutely necessary for the success of the experiment. In addition to the explanation of Mr. VOLTA's experiment we may observe, that though the direction of the Animal or Artificial Electricity is determined through the nerve alone; yet part of it may very probably go through the nerve to the muscle, and may thence return to the nerve; thus, when a man keeps his hand upon a charged Leyden phial, on making the discharge of the said phial, by the usual application of the discharging rod to the knob, and to the outside of it, the man will feel some effect of the discharge though he is out of the circuit of communication. — When the  
com-

communication is made between muscle and muscle, or between opposite parts of the same muscle, we may naturally remark that it is hardly possible to avoid touching a nerve; considering how much the nerves are dispersed and ramified through the muscular fibres.

41. The two legs of a frog being separated from the rest of the body, and skinned, the nerve of one thigh was disengaged from the muscles, and was left attached to the leg. Its extremity was coated. When the communication was made by means of the conducting rod between this coating and the middle between the two legs, both legs moved; but the motion of the opposite leg and thigh was very weak, in comparison with the motion of the leg to which the armed nerve belonged.

42. If a nerve be detached from the spine, and be left attached to the muscle, on applying the conducting rod to the armour of the nerve and muscle, a contraction of the depending muscles only will take place; but, says Dr. GALVANI, if the  
nerve

nerve be laid bare and not detached, then on forming the communication the whole body will be convulsed. This last observation, however, seems to be not generally true; the motions being almost always confined to the depending muscles; except when the experiment is tried with a living frog. But in this case the Animal Electricity is so powerful, as very easily to put the whole animal in motion.

43. It has been said, though I doubt the fact, that a small muscle, being separated from the body of a frog, was placed upon a silver coin. On touching it with the scissars in that situation, it shrunk up; but after this first contact, it moved whenever the scissars were only presented to it, without any actual contact. — In another muscle, similarly prepared, no motion could be excited by any mechanical stimulus; yet the application of the conducting rod caused it to move.

44. I took, says Dr. VALLI, a frog, which I divested of its integuments. I laid bare the spine, and divided it above the origin of the crural nerves, and also at the  
origin

origin of the lower extremities. Thus the frog was in two parts, communicating only by the crural nerves. These nerves I coated; and upon placing one of the branches of the conducting rod on the coating, and the other on the trunk, the lower extremities were instantly agitated as well as the upper part and fore-legs.—If the experiment be repeated when the nerve is tied, then no motion will take place in the lower extremities. If, instead of placing the conducting rod on the trunk, it be placed on the ovaries, liver, lungs, head, or fore-legs, the experiment answers equally well.

45. Prepare two frogs in the usual manner. Let the legs of one touch those of the other, and let the armed nerves remain at a distance from each other. Make the communication between the two armours of the nerves, and strong motions will be excited in the legs. This experiment seems to shew a double current of the Animal Electricity.—When the power of one of the prepared frogs is exhausted, then, on making the communication, the vigorous frog alone will move.

46. The

46. The application of Artificial Electricity will generally excite motions in those prepared frogs, whose Animal Electricity has been exhausted by the repeated application of the conducting rod. It is very remarkable, that the application of Artificial Electricity has sometimes revived in a great measure the Animal Electricity, so that afterwards motions could be again excited by the application of the conducting rod.

47. By the application of armours of different metallic substances, and forming a communication between them, the motions may be excited even in an entire living frog, and likewise in some other living animals, particularly eels and flounders. The experiment is performed thus: A living frog is placed upon a piece of zinc, with a slip of tinfoil pasted upon its back. This done, whenever the communication is formed between those two armours, especially when silver is used, the spasmodic convulsions are excited not only in the muscles which touch the metals, but also in the neighbouring ones. The  
slip

slip of tinfoil may be omitted when silver is used for the conducting rod. The experiment may be performed entirely under water.

48. This experiment may be made with a flounder in a similar, easy, and harmless manner. Take a living flounder, such as can almost always be found at the fish-mongers, lay it flat into a pewter-plate, or upon a sheet of tinfoil, and put a piece of silver, as a shilling, a crown piece, or the like, upon the fish. Then, by means of a piece of metal, complete the communication between the pewter-plate or tinfoil and the silver piece, on doing which the animal will give evident tokens of being affected.—The fish may afterwards be replaced in the water to preserve it for farther use.

49. Excepting frogs and the above-mentioned fishes, this experiment will hardly succeed with other living animals, unless part of the skin be removed. A lizard or a mouse, for instance, being fastened to a table by means of pins, or



otherwise, an incision must be made on its back as far as the flesh, and a piece of tin-foil must be applied to it. A similar incision must be made in another part, as the thigh or leg, and a piece of silver must be applied to it. Things being thus prepared, whenever the usual communication is formed between the two metals, the convulsions are excited, which, *cæteris paribus*, are stronger or weaker as the incisions happen to be made nearer to, or farther from, some principal nerve. And for the same reason, if in this experiment a nerve happen to be laid bare, and the metal be put in contact with it, the usual metallic communication will be attended with more violent movements. With insects that have a very dry outside, the incisions must be made very deep \*.

50. It

\* “Après avoir tranché la tête à la mouche, au papillon, au scarabé, &c. je leur fend, tout au long, le corcelet avec un canif, ou les petits ciseaux; et j’introduis profondément dans la fente, près du cou, un morceau de feuille d’étain, (le papier dit improprement argenté est très à propos) et un peu au dessous j’introduis, de même bien avant dans l’intérieur, le tranchant d’une lame d’argent, ou d’une petite mon-  
“noye :



50. It often happens in those experiments, and especially when performed with frogs and chickens, that the metallic application cannot excite any motions in the prepared limb, which however can be freely moved by the will of the animal. And, on the contrary, at other times the application of the conducting rod excites motions in limbs, which the animal seems to have no power of moving. Thus the application of opium to a muscle, or to a nerve, stops the voluntary motions of the muscle or muscles depending on that nerve, yet the application of the armours and metallic rod of communication, will produce motions in them.—There seems evidently to be in the animal frame a power of counteracting in a great measure the action, whatever that may be, of the metallic application.—When the animal is vigorous and upon his guard, the contrac-

“noyé : alors, quand j’avance celle-ci jusqu’au contact de la feuille d’étain, les jambes commencent à se plier, à se débattre, et les autres parties, et le tronc même, à s’agiter. Il est fort amusant d’exciter de cette manière le chant d’une cigale,” &c. VOLTA’s Experiments in the Phil. Transactions for the year 1793, p. 33.

tions can seldom be excited by this means; whereas, when a part of the body has been rendered previously more sensible by irritation, scarification, &c. then the application of the metals is attended with more considerable effects.

51. Even the living human body can be rendered sensible of the action of metallic applications, and both the senses of taste and sight may be excited by it. Let a man lay a piece of metal upon his tongue, and a piece of some other metal under the tongue, on forming the communication between those two metals, either by bringing their edges into contact, or by the interposition of some other piece of metal, he will perceive a peculiar sensation, a kind of cool and subacid taste, not exactly like, and yet not much different from, that produced by artificial electricity. The metals which answer best for this experiment are silver and zinc, or gold and zinc. The sensation seems to be more distinct when the metals are of the usual temperature of the tongue. The silver or gold may be applied to any other part of the mouth, to the nostrils, the ear,

ear, and other sensible parts of the body ; whilst the zinc is applied to the tongue, and on making the communication between the two metals, the taste is perceived on the tongue. The effect is more remarkable when the zinc touches the tongue in a small part, and the silver in a great portion of its surface, than *vice versa*.—Instead of the tongue, the two metals may likewise be placed in contact with the roof of the mouth as far back as possible, and on completing the communication between them, a strong taste or irritation is perceived. Mr. John Robison, in a letter to Dr. Fowler, gives the following curious observations : “ I had, *says he*, a number  
 “ of pieces of zinc made of the size of a  
 “ shilling, and made them up into a rouleau, with as many shillings. I find that  
 “ this alteration, in some circumstances,  
 “ increases considerably the irritation, and  
 “ expect on some such principle to produce  
 “ a still greater increase. If the side of  
 “ the rouleau be applied to the tongue, so  
 “ that all the pieces are touched by it, the  
 “ irritation is very strong and disagreeable.  
 “ This explains what I have often observ-

“ed, the strong taste of soldered seams of  
“metal. I can now perceive seams in  
“brass and copper vessels by the tongue,  
“which the eye cannot discover, and can  
“distinguish the base mixtures which  
“abound in gold and silver trinkets.”

And farther on he subjoins the following paragraphs: “Put a plate of zinc into one  
“cheek, and a plate of silver (a crown  
“piece) into the other, at a little distance  
“from each other. Apply the cheeks to  
“them as extensively as possible. Thrust  
“in a rod of zinc between the zinc and the  
“cheek, and a rod of silver between the  
“silver and the other cheek. Bring their  
“outer ends slowly into contact, and a  
“smart convulsive twitch will be felt in  
“the parts of the gums situated between  
“them, accompanied by bright flashes in  
“the eyes, and these will be distinctly  
“perceived before contact, and a second  
“time on separating the ends of the rods,  
“or when they have again attained what  
“may be called the striking distance. If  
“the rods be altered, no effect whatever is  
“produced.

“Care

“ Care must be taken not to press the  
 “ pieces hard to the gums; this either  
 “ hinders us from perceiving the convul-  
 “ sion, or prevents it. I find too, that one  
 “ rod, whether zinc or silver, is sufficient  
 “ for the communication, and even bring-  
 “ ing the two pieces together will do as  
 “ well, or perhaps better, but the rods are  
 “ easier in the management.”

52. Different persons are variously affect-  
 ed in this experiment; with some the  
 sensation, or taste, is either very slightly, or  
 not at all perceived, whilst with others it is  
 very strong and even disagreeable. Some  
 think it a mere pungency, and not properly  
 a taste. Mr. VOLTA says, he perceives a  
 sort of acrid, or somewhat an alkaline taste,  
 when the silver is applied to the apex, and  
 the tin to the back of the tongue; but  
 when the tin is applied to the apex, and the  
 silver to the back of the tongue, then he  
 perceives the subacid taste. Opium, spi-  
 rits, and other things which diminish the  
 sensibility of the tongue, will of course  
 render it less susceptible of the action of  
 metallic applications.

53. The communication between the two metals may be made various other ways, some of which may be more pleasant and satisfactory; place, for instance, two large glasses full of water contiguous to each other, but so as not to touch; put an oblong piece of tinfoil with one extremity into the water of one glass, and with the other extremity projecting out of it; —in the water of the other glass put one end of an oblong piece of silver, and let the projecting parts of those two metals touch each other; then dip the extremity of the tongue in the water of the first glass, and dip the fingers of one hand into the water of the second glass, on doing which, the subacid taste will be perceived, and will continue as long as the fingers and the tongue are kept in that situation.

54. In order to affect the sense of sight by means of metals, let a man in a dark place put a slip of tinfoil upon the bulb of one of his eyes, and let him put a piece of silver, as a spoon or the like, in his mouth. On completing the communication between the spoon and the tinfoil, a faint flash,



flash of white light will appear before his eyes. This experiment may be performed in a more convenient manner, by placing a piece of zinc between the upper lip and the gums as high up as possible, and a silver piece of money upon the tongue, or else by putting a piece of silver high up in one of the nostrils, and a piece of gold or zinc in contact with the upper part of the tongue, for in either of those cases the flash of light will appear whenever the two metals are made to communicate, either by the immediate contact of their edges, or by the interposition of other good Conductors of Animal Electricity.

55. When this experiment is performed by a person having his eyes open in a place in which there is very little light, a by-stander will generally perceive a slight contraction of the pupil whenever the metals are brought into mutual contact. The experiment will likewise answer, when one of the metals is placed between the gums and upper lip, whilst the other metal is placed between the gums and lower lip ; but in this case the flash appears to be diffused over the



the whole face, whereas in the other cases, it is confined to the eyes alone. By continuing the contact of the two metals, the appearance of light is not continued, it being only visible at the moment of making the contact, and very seldom when they are separated; it may therefore be repeated at pleasure by disjoining, and again connecting the two metals. When the eyes are in a state of inflammation, then the appearance of light is much stronger. This phenomenon is not alike perceived by every person, some being hardly sensible of it, whilst others observe a very strong flash.

56. In performing both the above-mentioned experiments, viz. that which produces the taste, and the other which produces the flash of light, some persons imagine to feel a gentle warmth diffuse itself over the tongue from its root to the very apex.

57. Besides the sight and taste, no other sense of the living human body has been affected by the application of different metals.—

tals \*.—It is necessary to observe, that in performing experiments with living animals, the various state and disposition of their bodies, produces a great variety of results; especially with living frogs, the effects are not always proportionate to the apparent strength and vigour of the animal, and sometimes they are even in the inverse proportion of it. Some persons have had pains produced by the application of metals into their mouths or ears. “ After  
 “ performing, *says Dr. Monro*, this experiment repeatedly, I constantly felt a pain  
 “ in my upper jaw at the place to which  
 “ the zinc had been applied, which continued for an hour or more; and in one  
 “ experiment, after I had applied a blunt  
 “ probe of zinc to the Septum Narium,  
 “ and repeatedly touched with it a crown-  
 “ piece of silver applied to the tongue, and  
 “ thereby produced the appearance of a  
 “ flash, several drops of blood fell from the

\* The application of different metals has been tried with persons that have undergone chirurgical operations, when a nerve has been laid bare, and in that case the contractions have been found to take place as in other animals.

“ nostrils;

“nostrils; and Dr. Fowler, after making  
 “such an experiment on his ears, observed  
 “a similar effect \*.”

58. It has been said, that most of the experiments mentioned in the preceding pages, especially those with living frogs, succeed best when surrounded by fewer persons, than when they are performed in a crowd. This observation seems to indicate, that the circumambient air may be concerned in it, but the following experiment seems to evince the contrary. The legs of a frog, prepared in the usual manner, were placed entirely under water in a clean glass vessel, and some oil was poured upon the water so as to cover the whole surface of it; the conducting rod was also plunged into the water, and was moved by means of a stick of sealing-wax, which passed through the water and oil. Though in this disposition the conducting rod and prepared legs were entirely under water, and the communication with the air was cut off by means of the oil and glass vessel, yet

\* Experiments on the Nervous System, p. 26.

whenever

whenever the conducting rod was pushed so as to touch the armour of the nerves and the muscles, the convulsions happened in all appearance as well as when the experiment is performed in air. Dr. GALVANI tried the same experiment in oil, but the movements did not take place\*. This failure might have been expected, considering that oil is not a conductor of this power, and that the above-mentioned experiments will not answer, even when the slightest film of oil happens to be interposed between the bodies which form the communication from the nerve to the muscle of a prepared limb.

59. Frogs killed in hot water, even when the water was not hotter than  $106^{\circ}$ , and then prepared, &c. have been found to retain a small portion of Animal Electricity, which however was of very short duration. Their power is not diminished by being killed in freezing water, or by the preparation being laid upon ice for some hours.

\* De Viribus El. in m. m. p. 22.

60. Chickens

60. Chickens and rabbits killed by drowning, and afterwards exposed to the action of metals, by applying the conducting rod to the muscles, and to a nerve previously laid bare and armed, have shewn various effects. In some every principle of motion was extinct, others shewed weak motions. Sometimes the convulsions were pretty strong, though not of long duration; and in some instances, by the excitation of those motions, the animal has been actually restored to life. It has been likewise observed in other instances, that animals which were almost dead, have been revived by exciting this influence.

61. Frogs killed by an electric shock, that is just sufficient to deprive them of life, and then prepared in the usual manner, are susceptible of the motions; but when they are killed by means of very strong shocks, and also when a very strong electric shock is sent through the nerve and muscle of the prepared limb of a frog, the motions will no longer appear.

62. Frogs

62. Frogs have been killed by laying bare the brain, and irritating it, or by applying opium to it. Frogs have been stupefied and rendered insensible of torture by the application of snuff. Dogs have been killed by means of hemlock and of arsenic. Frogs and some other animals have been killed by being confined in inflammable air, or nitrous air, or dephlogisticated air. Lizards have been poisoned with tobacco, and have died in convulsions. But in none of those instances the Animal Electricity was destroyed.—In the animals killed by confinement in the above-mentioned elastic fluids, the motions were very weak, and took place at great intervals of time.

63. Dr. FOWLER made the following curious experiment with opium. He made a tight ligature round the sciatic nerve of a frog, also divided the sciatic nerve of another frog, and then applied opium to their brains. After this preparation, he excited the motions in their legs by the usual application of the metals, and found that the leg, whose nerve had been tied or  
 § divided,



divided, continued to be contracted for a much longer time than the other leg.

64. Air vitiated by the combustion of sulphur, diminishes the effects of Animal Electricity, but in a less degree when the prepared frog is exposed to it, than when the living one is confined and suffered to die in it. In the latter case, the muscular fibres sometimes become lax and soft; in both cases the motions are weak, and vanish very soon.

65. The moving power of the prepared legs of a frog, is much diminished by being kept in a vessel of nitrous air for a certain time, and is entirely destroyed by a longer continuance in that elastic fluid.—Inflammable air acts in a similar manner, though not so powerfully. In those cases the muscles do not appear to have suffered any alteration.—Perhaps the nerves alone are affected by it.

66. Animals starved to death, or killed by means of corrosive sublimate, and afterwards



wards prepared and subjected to the action of metals, have shewn no motion whatever.

67. The facts which we have noticed above, shew, that in animals dead as well as living, the faculty of being put in motion, of being convulsed, &c. by the application of metals and other bodies, possesses but one characteristical property in common with electricity; viz. its being conducted by certain bodies, and not by others. Upon the whole, the Conductors of the one are also Conductors of the other; yet this law is not without some remarkable exceptions. The convulsion occasioned by the application of metals, is indeed analogous to the electric shock, but that convulsion is likewise the effect, though in a more limited manner, of other stimuli. The other two peculiar properties of electricity, namely, the light, and the attraction and repulsion, have not been discovered in the muscular power or Animal Electricity of living or dead animals. Upon the supposition that it is the same thing as electricity, the want of light may perhaps be attributed

to its very rarified state and small quantity. But with respect to the attraction and repulsion, assertions have been published of those properties having been actually discovered; I believe, however, that those appearances of attraction and repulsion must be attributed to other causes, as the following paragraphs will shew. It has been said, that fourteen frogs having been prepared in the usual manner, the armours of all their crural nerves were connected together, and the same thing was done with the muscles of all their legs; the communication was then made between those two armours, viz. that which communicated with the muscles, and that which communicated with all the nerves; in doing which two bits of straw, which happened to lie near the circuit of communication, were attracted by it.—*Le Journal de Physique* likewise mentions, but without describing the method, that undoubted proofs of repulsion, occasioned by Animal Electricity, had been observed with an electrometer. I also find recorded, that the hair of a mouse prepared for those experiments, was observed to move whenever a Conductor was presented to it,  
and

and besides, that it moved when situated near the armour of a combination, or battery, of several prepared frogs \*.

68. With respect to this last observation, whoever is conversant with electrical experiments, will naturally remark, that the hair of certain animals is so easily electrified by the slightest friction, and continues so long in that state, that the above-mentioned appearances of attraction or motion, may with much more propriety be attributed to the common than to the Animal Electricity. But the other assertions being more positive and less equivocal, induced Dr. LIND and myself to put the subject to the test of actual experiments.

69. For this purpose we prepared six frogs in the usual manner, and laid their legs all parallel upon silver plates, which rested upon a pane of glass. A silver wire was placed in contact with the tinfoil armours of all their crural nerves, and this wire was raised above the glass by means of

\* VALLI's Exp. on An. El. p. 112.

sealing wax. With this preparation, the completion of the communication between the two armours was formed various ways. We placed a pendulum of gold leaf very near the circuit; we placed the pendulum itself, which was exceedingly sensible, in the circuit of communication; one of the metallic Conductors coming within about one-fiftieth part of an inch of its extremity; we also employed a very sensible electrometer, disposing it in various situations: but in none of those cases could we discover the smallest sign of attraction or repulsion; neither could the power be transmitted when the smallest interruption existed in the circuit of communication.— We made a cut with a pen-knife across a piece of tinfoil, fastened upon a stick of sealing-wax. This interruption could hardly be so great as the 200th part of an inch, and certainly it was not greater. We attempted to form the communication by means of this apparatus in a very dark room, in hopes of discovering the spark, but no light whatever could be perceived, and indeed it could hardly be expected, considering that this small interruption was quite sufficient to  
prevent

prevent the motions, or the communication of that power, which, for want of a better name, we call Animal Electricity.

70. The principal phenomena of Animal Electricity, viz. the property of being put in motion by a metallic or other communication made between the nerves and the muscles, is not peculiar to a few animals only, but seems to be a property of all animals in general; a law of nature, which admits of few exceptions, and even those exceptions are of a very doubtful nature. The experiments have already been tried with a great variety of terrestrial, aerial, and aquatic animals. The human body, whilst undergoing certain chirurgical operations, or its recently amputated limbs, have been convulsed by the application of metals. From the ox and the horse down to the fly, the effects of metallic applications have been repeatedly and unequivocally observed. With some the power lasts longer than with others; the movements also are more or less evident and powerful, according to the various nature and disposition of the animals. The leg of a recently dead horse



was agitated so violently by the application of a shilling and a bit of tinfoil, that the strength of a robust man was unable to check the blow. Animals possessed of cold blood, are in general more retentive of that power than those which have hot blood; but amongst those of the same class a considerable variety is observable, which arises from the different strength or irritability of their fibres, and probably from other causes that are as yet unknown. The animals which form an exception to the above-mentioned general law, are several worms, some other insects, the oyster, and a few other small sea animals. But as the organization of those animals seems not to be possessed of much sensibility, nor admits of much motion, it may be presumed that the effects of the metallic application are only too weak to be perceived by our senses; and in fact several animals, which some time ago were thought not to be affected by the contact of metals, have been lately caused to contract in consequence of the discovery of more active metallic combinations, or of some of their more sensible parts.

71. The preceding pages contain all the remarkable facts that I have been able to collect, relative to a subject which is likely to become of great importance. Those surprising effects of an unknown cause, generally inexplicable, and sometimes contradictory, seem to admit of no theory sufficiently probable or satisfactory, nor can we yet see how they may be applied for the benefit of mankind. An attentive consideration of the subject will naturally suggest several doubts and queries, which can only be answered by future experiments and discoveries.—In what manner does artificial electricity affect the muscles?—Does it act as a mere stimulus or otherwise?—Where is the Animal Electricity generated, and by what mechanism is it transmitted from one part of the body to another?—Does it proceed from the brain, or is every nerve actuated with that generating power?—What reason can there be for the necessity of using two different metals?—And after all, are those phenomena really the effects of electricity, or of some other unknown fluid *sui generis*?



72. The want of several of the characteristic properties of electricity, may perhaps be owing to the weak state of that power in animals, and therefore it would be unphilosophical to admit another agent as the cause of those muscular motions, contractions, &c. unless a property of it could be discovered, which is absolutely repugnant to the ascertained laws of electricity. In that case we might with propriety say, that as there are several liquids or visible fluids like water, spirits, &c. which have diverse properties in common, at the same time that they are essentially different; that as there are several invisible and permanently elastic fluids like common air, inflammable air, fixed air, &c. which are very dissimilar, though possessed of certain common properties; so there may be several sorts of more subtle fluids essentially different from each other, yet bearing some analogy to the electric fluid.

73. Having, towards the beginning of this account, shewn the possibility of the electric fluid existing in an unbalanced state amongst the various parts of the animal body, I shall  
conclude

conclude with a few remarks concerning the origin of the accumulation or rarefaction of that fluid in general, which may probably promote the investigation of this curious subject.

74. There is a well known and very extensive law in the science of electricity, which is, that the mere proximity of an electrified body, is sufficient to induce a contrary electricity in another body, without its losing any part of its own\*. Upon this principle, if the permanent existence of a quantity of electricity in any place be admitted, one may easily conceive how other bodies may be electrified by it, and also how the electricity may thereby be accumulated to any degree. But it will naturally be asked, where is that electrified body, the first term of the series, from which the accumulation may be derived?—To this I answer, that strictly speaking, the common notion of the electric fluid existing in a balanced state amongst the bodies of

\* The action of Mr. VOLTA's Electrophorus, and of the Doubler of Electricity, are intirely depending upon this law.

our globe, is by no means true. Great quantities of electricity accumulated on bodies that are not absolutely insulated, will be readily dispersed amongst the surrounding bodies, in the same manner as a quantity of water, which is poured out of a vessel upon any surface, will soon find its level, by descending from the highest to the lowest places. But let a man try to remove the last drops of water, or particle of moisture, from the inverted vessel, and he will find it very difficult to succeed. In like manner those persons, who are accustomed to make nice electrical experiments, know how extremely difficult it is to remove small residuums of electricity from a Leyden phial, from a piece of wood and other bodies, which have been once electrified. It is evident, therefore, that a beginning of electric accumulation is by no means difficult to be found. But, independent of this remark, if we consider that electricity is generated by evaporation, condensation, rarefaction, friction, and other causes; and that those natural processes happen continually and in every place, we must then conclude, that, far from remaining in a balanced or level state,

state, the electric fluid must be continually fluctuating amongst the various substances of our globe. It is accumulated in some and rarefied in others; the accumulation is removed from the latter to the former, and perhaps it seldom happens that two bodies of similar shape, bulk, and substance, contain exactly equal quantities of electric fluid. This accumulation and rarefaction of it, this positive and negative state, is in most cases too small to affect our electrometers and other instruments; but the effects of very small quantities of artificial electricity upon animals, shew that it is by no means too small for the mechanisms framed by the most exquisite hand of Nature.

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HISTORY  
OF THE  
DOUBLER OF ELECTRICITY.

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ON the 10th of May, 1787, the description of a new electrical instrument was read at the Royal Society, and was afterwards published in the 77th vol. of the *Philos. Trans.* This instrument was by its inventor, the Rev. A. Bennet, named a *Doubler of Electricity*, or a machine by which the least conceivable quantity of positive or negative Electricity may be continually doubled, till it becomes perceptible by common electrometers, or in visible sparks.

It consists of three circular brass plates, A, B, and C, all of the same diameter, which is about three or four inches. A is polished and thinly varnished on the upper surface;

surface; B is polished and varnished on both sides, and is furnished with an insulating handle, which is fastened to its circumference; C is polished and varnished on the under side, and is furnished with an insulating handle, that rises perpendicular to the plate from the centre of its upper surface.

The method of doubling, quadrupling, &c. a small quantity of electricity with this instrument, is the following: The plate A is placed horizontally upon an insulating stand, with its varnished surface upwards, and the plate B is laid upon it. The small quantity of electricity which is required to be doubled, whether from the atmosphere, or otherwise produced, must be communicated to the under side of the plate A, and at the same time the plate B must be touched with a finger; in consequence of which, the last-mentioned plate will acquire an electricity contrary to that of the plate A. This done, the plate B must be lifted up by its insulating handle; the third plate C must be laid upon it, and in that situation C is touched with the finger, in consequence



quence of which the plate C will acquire an electricity contrary to that of B, and of course of the same sort with that of A. Now the plates B and C, being held, by their insulating handles, must be separated from each other. B must be laid upon A, and must be touched with the finger, whilst C is made to touch with its edge the under side of the plate A. By this means, the original quantity of electricity is doubled once, as may be easily comprehended after a little consideration, and by repeating the above-described operation, it may be doubled again and again to any degree, viz. the plate B being removed from over the plate A, the third plate C is laid upon it, and C is touched with a finger; then these two plates are separated, B is replaced upon A, and is touched with a finger, whilst C is kept in contact with the lower side of it, and so on.—This effect depends upon the two well-known laws of Electricity, viz. first, that a body presented to another body electrified, acquires thereby the contrary electricity, provided it be made to communicate with other bodies or with the earth; and secondly, that the capacity of a body for hold-



ing electricity, is increased by being placed with its surface contiguous to another body not insulated.

The advantages to be derived from an instrument of this sort appeared at first sight so striking, that I soon procured one of them, and used it for the discovery of small, and otherwise imperceptible quantities of electricity ; but a few trials soon shewed, that no dependence was to be placed upon such an instrument, the use of which was generally, if not always, attended with equivocal results. Sometimes it shewed different electricities after similar operations, and at other times it shewed the very same sort of electricity, when, from the difference of the original communication, it ought to have shewn different electricities. But its more remarkable fault was, that in a number of experiments, it generally continued to shew that same sort of electricity which had been communicated to it in the first experiment.

It naturally occurred to me, that the defect arose from the contact of the varnished surfaces

surfaces of the plates, which might in some measure electrify them ; and from their retaining a portion of that sort of electricity, which happened to be communicated to them in the first experiment. In consequence of this consideration, I constructed a doubler which could perform the same office, but in a less exceptionable manner. It consisted of three large and flat tin plates not varnished, and each of them was supported in a perpendicular situation on a separate glass stand. They could be either placed facing each other at any required distance, without touching, or could be made to touch each other edgewise. With these plates A, B, and C, the same operation could be performed as with the above described doubler, viz. A and B were placed facing each other at the distance of about one-tenth of an inch, and the original small quantity of electricity was communicated to A, whilst B was touched with a finger ; then B being removed from the vicinity of A, was placed facing the plate C, and in this situation C was touched. After this the plate C was situated with its edge in contact with the back part of A ;

B was

B was put facing A, and in that situation was touched, &c.

As the plates of this instrument were not varnished, the possibility of exciting any electricity was removed, and in fact the effect seemed to be somewhat more constant, yet it was far from being uniform and certain. It appeared, in short, that without communicating any electricity to it, the instrument after doubling a few times became always sensibly electrified. When the instrument had not been used for some days, then, in order to make it produce that spontaneous sort of electricity, it was necessary to repeat the operation of doubling at least 20 times; but in any subsequent trial after this first experiment, a manifest degree of electricity could be produced by doubling about 12 times, and this electricity was almost always of the same sort as in the first experiment.

The difficulty now consists, in finding out the origin of that electricity which is produced by this instrument, without the apparent communication of any quantity of

it. In my opinion it must originate either from the surrounding air, or from the instrument itself, and probably from both causes. For instance, when the instrument has remained a long time unemployed, its plates are then likely to be in the same state of electricity, and therefore that plate, which in the beginning of the operation remains untouched, will probably acquire some electricity from the surrounding air, and of course that electricity will be doubled until it becomes manifest. But when the plates have thus been once electrified, it is then extremely difficult, if not impossible, to deprive them of some residuum of electricity, which they seem to retain even after one or two days rest; at least I have not been able to succeed in the attempt of removing it. The residuum adheres probably to that part of the insulating stand which is contiguous to the metal plate, from whence even the flame of a candle is not sufficient to remove it. To this residuum then the origin of the electricity, in the trials after the first, must be generally attributed. Sometimes the electricity communicated by the atmosphere, may be much

stronger than the residuum that adheres to the plate, and then of course the electricity of the atmosphere will be manifested by the operation of doubling ; but as the quantity of residuum, or adhesive electricity, is variable, as well as the electricity of the surrounding air, or of any other process under consideration, therefore one can never be sure of the effects resulting from the use of such an instrument.

My objections to the use of the Doubler, even when made in the above-mentioned less exceptionable manner, were read at the Royal Society in November, 1787, and were afterwards published in the 78th vol. of the Phil. Transf. On the 5th of June following, the description of an improved Doubler by Mr. WM. NICHOLSON, was read at the Royal Society, and was afterwards published in the 78th vol. of the Phil. Transf. Part II.—Mr. NICHOLSON adapted the three plates A, B, C, to a mechanism, in which nothing more was required to be done than to turn a winch ; for by the very ingenious construction of its parts, in every revolution, the whole operation of once

G 2

doubling

doubling was accomplished, a second revolution quadrupled the electricity, and so on. In this machine the plates do not touch one another, but only come with their surfaces very near each other, exactly as I had done with the three separate plates. Mr. NICHOLSON very properly calls this machine, “an instrument, which, by the turning of  
“ a winch, produces the two states of elec-  
“ tricity without friction, or communica-  
“ tion with the earth.” He acknowledges the propriety of my supposition, concerning the origin of the electricity in this instrument, viz. that it is generally owing to some residuum adhering to its parts, and concludes with the following just observation: “ But here, as well as in the common  
“ Doubler, the effect is rendered uncertain  
“ by the condition, that the communicated  
“ electricity must be strong enough to de-  
“ stroy and predominate over any other elec-  
“ tricity the plates may possess. I scarcely  
“ need observe, that if this difficulty should  
“ hereafter be removed, the instrument will  
“ have great advantages as a multiplier of  
“ electricity in the facility of its use, the very  
“ speedy



“ speedy manner of its operation, and the  
 “ unequivocal nature of its results.”

Mr. BENNET, in his New Experiments on Electricity, published in 1789, page 84, describes a method of depriving the Doubler of its adhesive or spontaneous electricity. It consists in turning the handle round a considerable number of times, whilst all the three plates of the instrument are communicating with the ground by means of wires. But I must confess that I could not upon trial, find this method sufficient to produce the desired effect.

Notwithstanding those objections, several discoveries are said to have been lately made with it, the most remarkable of which are the following; and the Doubler is at present by some persons, considered as one of the most useful instruments in electricity.

In a paper of Mr. J. READ, published in the Phil. Transf. of the present year 1794, several experiments are related, which concur to establish the following general proposition,

position, viz. that common atmospheric air, when vitiated even in a small degree by respiration, putrefaction, and other natural processes, shews a deficiency of electric fluid; whereas in its natural pure state, it is generally possessed of positive electricity. The facts upon which Mr. READ establishes this proposition, were proved by means of the Doubler; for when he worked it in a close room, in which people had been shut up for a certain time, or upon a dunghill, or in a privy, he obtained negative electricity; but when he worked it in the open and clear air, the result was positive electricity.

“ Knightsbridge charity-school, *says he in*  
 “ page 269, fills up a piece of ground be-  
 “ tween the north-end of the chapel and  
 “ Hyde Park wall, and the main sewer of  
 “ that neighbourhood runs at no great  
 “ depth under it; the number of children  
 “ educated in this school, is thought by  
 “ some to be too great for the size of the  
 “ school; on these accounts it becomes in-  
 “ fected with a very disagreeable stench,  
 “ especially when the door and windows  
 “ are

“ are shut up; I have sometimes found the  
 “ noxious effluvium so very strong in this  
 “ school, that I have hastened out to breathe  
 “ a purer air. I have often examined the  
 “ electrical state of the air in this school  
 “ with the Doubler, and have always found  
 “ it strongly negative; which shewed that  
 “ the aqueous or other conducting matter  
 “ lodged in the air of the school, possessed  
 “ less than their natural quantity of elec-  
 “ tricity, while that of the school-master’s  
 “ parlour adjoining, having nobody in it,  
 “ possessed somewhat more than its natural  
 “ quantity; it was found therefore positively  
 “ electrified.

“ July 5th, therm.  $76^{\circ}$ , I went to the  
 “ school, and found the door and windows  
 “ set wide open to let in cool air; I now  
 “ perceived no stench at all in the school,  
 “ and thought it needless to try it. How-  
 “ ever the school-master observed, that the  
 “ further end of the school was at all times  
 “ most infected with, and seldom quite clear  
 “ of stench. I therefore worked the Dou-  
 “ bler in that part of it, and after a very  
 “ few turns it became electrified negatively,  
 “ rather against my expectation. I then

“ tried the other end of the school, which,  
 “ by the door being wide open, was less  
 “ liable to retain any noxious effluvium,  
 “ and there the Doubler gave positive elec-  
 “ tricity. After this I tried it in the school-  
 “ master’s parlour, where it was positive  
 “ also.”

Upon a dunghill, Mr. READ made the  
 following experiments: “ I first, *says he*,  
 “ went upon the highest part of the dung-  
 “ hill, and held the Doubler in one hand a  
 “ little above the dung, yet within the reach  
 “ of the ascending pale-coloured vapour,  
 “ and with the other I turned the revolving  
 “ plate five or six times, by which means  
 “ the Doubler became electrified positively;  
 “ which was known to be at that time the  
 “ general electrical state of the atmosphere.  
 “ —The Doubler was now lowered, and  
 “ placed upon the dung; and also it be-  
 “ came charged by a few turns of the re-  
 “ volving plate, which made me suspect  
 “ that I had not completely cleared the  
 “ Doubler of its former charge; however,  
 “ on trial it was found to be electrified ne-  
 “ gatively.

“ I re-

“ I repeated these experiments with the  
 “ Doubler, placed on various parts of the  
 “ dunghill, without any remarkable variation  
 “ in the results, except that the negative state  
 “ of the exhaled vapour was stronger, and  
 “ extended to a greater distance, in the de-  
 “ pressed or hollow parts of the dung, where  
 “ the vapour was less exposed to a mode-  
 “ rate gale of wind than then prevailed, than  
 “ on the open parts which lay full to it.”

In the additions at the end of Dr. DAR-  
 WIN'S Zoonomia, I find the following pas-  
 sage: “ By the experiments published by  
 “ Mr. BENNET, with his ingenious Dou-  
 “ bler of Electricity, which is the greatest  
 “ discovery made in that science since the  
 “ coated jar, and the eduction of lightning  
 “ from the skies, it appears, that zinc was  
 “ always found minus, and silver was al-  
 “ ways found plus, when both of them  
 “ were in their separate state.”

Not knowing of any other experiments  
 published by Mr. BENNET on this subject,  
 I imagine that the above-mentioned para-  
 graph alludes to the experiments related in  
 Section

Section VII. of Mr. BENNET's New Experiments, &c. The title of this section, is

“Experiments on the adhesive Electricity of  
“Metals, and other conducting Substances.”

Those experiments are performed in the following manner: The Doubler is first of all deprived of its natural electricity, as far as that object can be obtained, by turning the handle about 50 times round whilst the plates communicate with the ground. The communication is then removed, and whilst the revolving plate stands opposite to one of the fixed plates, two different metallic substances are applied, one to the former, and the other to the latter of those plates. The handle being then turned round, after a various number of revolutions, the instrument becomes electrified sufficiently to affect an electrometer. The experiment is also performed, by applying a single substance to one of the plates. Mr. BENNET found that the electricity produced was different, according to the metal that was applied. But from the account of his own experiments, and much more from the experiments of other persons, the results are not constant.



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Of the METHODS of

Manifesting the PRESENCE, and ascertaining the QUALITY,  
of small Quantities of

Natural or Artificial ELECTRICITY.

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THE persevering industry of many ingenious persons in the philosophical world, since the beginning of the present century, has gradually, and with considerable success, examined the effects, ascertained the properties, and investigated the laws of a science almost entirely unknown before. Various wonderful phenomena of nature have been explained, means have been discovered of avoiding some of their pernicious effects, and a greater insight into the general operations of nature, has been derived from the knowledge of so great and so extensive a power as is commonly understood under the name of *Electricity*; yet, if instead of contemplating its more striking

ing phenomena, viz. those which are apt to surprize more by the magnitude of their effects, than by the instruction which they may afford, we endeavour to examine the subject more closely, we shall find that our knowledge of Electricity goes very little, if at all, beyond the superficial part of it. We know, for instance, that a piece of glass, or other electric when rubbed, will produce that power called Electricity, that the glass will communicate the acquired electricity to a piece of metal, that the piece of metal will retain that power in certain circumstances, and so on ; but no person has shewn how such power is generated by the friction, or what prevents its passage through the substance of some particular bodies. It has been ascertained, that the air of most countries, and probably of the whole world, as well as the clouds, fogs, rain, &c. are almost always electrified ; but we are ignorant of the office which this electricity can have in the great laboratory of nature ; for surely so great and so active a power, can hardly be intended by the great Author of the universe, merely to intimidate mankind with the thunder and the lightning.

lightning. Those persons, therefore, who are now willing to distinguish themselves in this interesting branch of natural philosophy, ought to examine the electrical power, not so much in its accumulated, as in its incipient state. If its first origin could be investigated, it would be afterwards very easy to comprehend its increase; for we may easily understand how a great quantity of it may be accumulated, from the often repeated additions of its smallest portions.

This truth having been understood by several philosophical persons, induced them to contrive instruments necessary for the purpose, and to make numerous observations, which have undoubtedly promoted the knowledge of the subject; but a vast deal still remains to be done in order to ascertain the origin, the nature, and the various uses of electricity.

The various instruments and methods contrived for this purpose, may be comprehended under two general heads, viz. 1st, those which shew the Electricity in its natural

tural quantity and quality ; and 2dly, those which manifest the electricity in a sort of unnatural state, as by condensing, multiplying, or changing it in contrarium. The instruments of the first class are the electrometers, which have been described in the first and second volume of this treatise, excepting however the gold-leaf electrometer, which we shall describe before we proceed to examine the instruments of the second class.

This electrometer was invented by the Rev. A. BENNET\*. It consists of two slips of gold-leaf suspended to the cover of, and hanging within, a cylindrical glass vessel. It is in short like my bottle electrometer, excepting that slips of gold-leaf are used, instead of the corks suspended by fine silver wires. The slips of gold or silver leaf are about three inches long, and sometimes they are made narrower at the lower than at the upper extremity. This electrometer is certainly the most sensible instrument of the kind, and very useful in nice experi-

\* See the Phil. Transf. vol. 77.

ments,

ments, it being affected by very small quantities of electricity, in a ready and unequivocal manner \*, but it is by no means portable.

As it is rather difficult to fasten the slips of gold-leaf to this electrometer, and to cause them to hang parallel, I have contrived a method which remedies that defect. When the slips are cut and are lying upon paper, or on the leather cushion upon which they are cut, I make them equal in length, by measuring them with a pair of compasses, and cutting off a suitable portion from the longest; I then cut two bits of very fine gilt paper, each about half an inch long, and a quarter of an inch broad, and by means of a little wax, stick one of them to one extremity of each slip of gold-leaf, so as to form a kind of letter T. This done, I hold up in the fingers of one hand, one of those pieces of paper with the gold-leaf

\* The electrometers made with cork or pitch balls, are subject to an inconvenience, which does not take place in the gold-leaf electrometer; it is, that sometimes when they are electrified, the balls adhere to each other for a considerable time before they will separate, and then they separate with a kind of jirk all at once.

suspended

suspended to it, and hold the other with the fingers of the other hand; then bringing them near to each other, and having adjusted them properly, viz. so as to let them hang parallel and smooth; I force the pieces of paper which now touch each other, between the two sides of a sort of pincers made of brass wire, or of very thin and hammered brass plate, which pincers are fastened to the under part of that piece which forms the top or cover of the glass vessel. As these gold slips are very apt to be spoiled, I keep several of them ready cut in a book, each having a cross piece of paper fastened to one extremity, so that in case of accident, a new pair of gold slips may be soon put between the aperture of the above-described pincers; and by this means the electrometer is rendered, in a certain manner, portable.

Of the other sort of instruments useful for discovering the presence and quality of small quantities of electricity, several have already been described in the preceding part of this treatise. In pages 41 and 65 of Vol. II. the reader may see how a weak  
and



and diffused electricity may be manifested by exposing an electrometer, or an electrophorus plate to it; for these will thereby acquire the contrary electricity agreeably to the laws of electric atmosphere. In page 259 and following of the same vol. is described Mr. VOLTA's method of condensing the diffused and otherwise imperceptible quantity of electricity upon a metal plate, which will afterwards electrify an electrometer very sensibly. But it remains now to describe the methods of rendering perceptible a quantity of electricity, which is in itself too small either to affect an electrometer or Mr. VOLTA's condenser; for, in fact, this condenser is only capable of collecting together a quantity of electricity, which, though not very small, is however so little condensed, or so diffused through a proportionably large space, as not to be capable of affecting an electrometer: hence, if by means of this apparatus the experimenter expected to render the electricity of a small tourmalin, or that of a hair when rubbed, more manifest than it generally is, when communicated immediately to an electrometer, he would find himself mistaken.

The doubler of electricity is the instrument which is capable of increasing a very small quantity of electricity to a degree more than sufficient to manifest its quality; but the properties and defects of that instrument have been sufficiently noticed in the preceding pages. I endeavoured, in consequence of those observations, to construct an instrument which might answer that purpose in an unequivocal manner, and after some fruitless attempts, I at last succeeded in the invention of one, which is not subject to equivocal results. It renders a small quantity of electricity manifest, by continually adding portions of the contrary electricity to an insulated plate, until that plate becomes so far charged with it, as to electrify an electrometer, which of course will manifest an electricity contrary to that originally communicated to the instrument. With this machine, which by way of distinction we shall call the *Multiplier of Electricity*, the accumulation of the communicated power does not advance by any means so quick as with the doubler; but the experiments contained in the following

pages will convey a much better idea of its performance.

The figures 3d and 4th represent this new instrument, and they are about two-thirds of the real size. Q R S is the bottom board, upon which are steadily fixed on the glass sticks H, G, two flat brass plates A and C.—B is a similar brass plate supported by a glass stick I, which is cemented in a hole made in the wooden lever K L, which moves round a steady pin K, that is screwed tight in the bottom board. By moving this lever backwards and forwards, the plate B may be alternately put in the two situations represented by the figures. N is a thick brass wire fixed tight into the bottom board. O M is a crooked wire that proceeds from the socket on the back of the plate B.—There is a fourth brass plate D, similar to the other three, which is supported not by glass, but a wire; and this wire is screwed fast to an oblong piece of brass F P, that slides in a groove made for the purpose in the bottom board Q R S; so that by applying a finger's nail to the notch on the end F, the sliding piece F P

may be drawn out either intirely or to a certain length, and of course the plate D will be removed to any required distance from the fixed plate C. I need not say any thing particular respecting the sockets of those brass plates, they being clearly indicated in the figures, excepting only that the socket of the plate A reaches as high as the top of it, and serves to receive a wire, or other apparatus, in certain occasions \*.

The parts of this instrument are so adjusted, as that when the lever is in the situation of fig. 3d, viz. is pushed as far toward Q as it can go, the plate B comes parallel to the plate A, and about one-twentieth of an inch distant. At the same time the extremity of the wire OM just touches the fixed wire N, and of course renders the plate B un-insulated. But as soon as the lever begins to be moved towards S, the communication of the plate B with the wire

\* I have not been particular in mentioning the real dimensions of the parts of the instrument, as being not material. The plates of mine are an inch and a half square; but I think that it would be better if they were a little larger.

N, or with the ground, becomes interrupted, and B remains insulated. And when the lever has been moved as far as it can go towards S, the wire M comes in contact with the plate C, as shewn in fig. 4th. Then the two plates B and C communicate with each other, though they are otherwise insulated. The fourth plate D being supported by a wire, communicates with the ground; and when the sliding piece P F is pushed home, it stands parallel to, and at about one-twentieth of an inch from, the plate C.

When the instrument is situated as in fig. 3d, if an electrified body be brought into contact with the plate A, this plate will imbibe a great deal more of that electricity than it would otherwise, because its capacity is increased by the vicinity of the un-insulated plate B, and therefore, if after the communication of that electricity, the plate B, by moving the lever, be removed from that situation, and A be made to touch an electrometer, this will be electrified more sensibly by it, than it would have been by the contact of the original electrified body

H 3

itself.



itself. So far the plate A acts like a condenser, or collector, of electricity. But let us now consider the instrument as a multiplier.

When the plate A has received a small quantity of electricity by the contact of any electrified body whatever, and that body is removed, the plate B being un-insulated and opposed to the electrified plate A, will, like the metal plate of an electrophorus, acquire the contrary electricity, by either receiving from, or giving to, the ground some electric fluid, according as the plate A happens to be electrified. Thus, suppose that A has been electrified positively, B will become negative, and *vice versa*. If now the lever be pushed towards S, the plate B will remain electrified negatively, the communication with the ground being cut off; and when B comes into the situation represented by fig. 4th, at which time the wire M touches the plate C, the negative electricity of B will go to C, because the capacity of C for holding electricity is considerably augmented by the vicinity of the un-insulated plate D. If after this the lever be  
moved



moved back again to its first situation, B will be made negative a second time in the same manner as before: and by pushing the lever again towards S, that second charge of negative electricity will be communicated from B to C; and thus, by repeating the operation, which consists in merely moving the lever backwards and forwards, a considerable quantity of negative electricity will be accumulated upon C.

In fact, the action of this instrument resembles very much that of an electrophorus; for the plate A may represent the excited resinous plate, B may represent the metal plate of the electrophorus, and C is a kind of reservoir, into which the successive charges of the plate B are collected.—When a number of those charges or portions of electricity has been communicated to C, if the sliding piece F P be drawn out about an inch, and of course the plate D be removed to the like distance from the plate C, the capacity of the plate C will thereby be much diminished: and therefore if an electrometer be brought into contact with it, the electricity will be manifested:

nifested: whereas the electricity originally communicated to the plate A, could not have affected an electrometer in any sensible degree.

In using this instrument, 30 or 40 additions of electricity are the utmost number practicable; for after that number the augmentation of the charge upon C will not go any farther; the limit of which is, when the charge of C is increased to such a degree, as to leave a portion of electricity upon B, equal to that portion which B can receive from the action of A.

In this case, let C touch an electrometer as mentioned above, and if the electrometer does not diverge, proceed to a second process; for though its pendulums do not diverge, yet some electricity remains in them, which must not be disturbed, as it will help the effect of the second operation, which is as follows: Push in the slider FP, and go on moving the lever backwards and forwards as before, by which means, after a certain number of additions, the plate C will acquire a second charge, about as high as the former; and if then the slider FP be  
pulled

pulled out, and C be brought into contact with the same electrometer, the divergency of the pendulums, which before was either not at all or hardly perceptible, will thereby be rendered more conspicuous: and thus it may be increased still farther by a third and a fourth operation. But if, notwithstanding those repeated operations, the electrometer should be found not to diverge, the quantity of electricity may still be augmented by another method, which is, by communicating that little electricity of C to the plate A of another instrument of the same sort, and proceeding with that in the manner already described.

The principal cause, which renders this instrument certain in its effects, is, that all the residuum of electricity, which can possibly remain upon the plate A, is always too inconsiderable to produce any bad effects; the electricity being never accumulated upon this plate, but constantly remaining in its original quantity, or rather in a decreasing state: whereas in the doubler the electricity is increased to a prodigious degree upon the plate that received the original quantity;

tity; and as the residuum must be greater or smaller, according to the charge of the plate, though not exactly in that proportion, it follows, that the residuum left upon the receiving plate of the doubler may be actually greater than the quantity of electricity, which was first communicated to it.—I shall now describe a few experiments made with this multiplier, in order to render both the management of the instrument more familiar, and its degree of sensibility more evident; previous however to this, it will be necessary to remind the reader of two things, viz. first, that the electricity communicated by the plate C to the electrometer is contrary to that possessed by the plate A; and, secondly, that the meaning of four, five, or any number of additions, is, that the lever L K has been moved four, five, or any other number of times from the situation of fig. the 3d to that of fig. the 4th. But when it is said that four, five, &c. more additions produced a greater divergency, the meaning then is, that after having communicated the first charge of C to the electrometer, the plate D was replaced near C, the lever was moved back-

wards

wards and forwards four or five times more, and then the second charge of C was likewise communicated to the same electrometer.—It is almost superfluous to observe, that after having performed one experiment, it is necessary to touch with a finger all the three plates A, B, and C, in order to deprive them of the electricity acquired in the operation, and to render the instrument fit and ready for a second experiment.

Exp. I. An electrometer was electrified positively in a small degree; then time was allowed for that electricity to dissipate till the pendulums of the electrometer appeared just to touch each other, and when this happened, the plate A was brought into contact with the electrometer, so as to receive from it part of that small quantity of electricity. This done, the lever was worked, and after ten additions the plate C was found capable of electrifying an electrometer sufficiently to shew, that the electricity was negative. Ten additions more increased the divergency of the pendulums, and ten more made them strike against  
the



the glass bottle in which they were contained.

Exp. II. About a tea-spoonful of water was poured upon a burning coal, and immediately after an insulated tin plate of eight inches in diameter was held over it for about five or six seconds of time, to catch and condense the vapour. This plate was then brought into contact with the plate A of the multiplier, and after four additions the electrometer was sensibly electrified by the contact of the plate C. Four additions more increased the divergency of the electrometer to a degree more than sufficient to shew that its electricity was negative, and consequently that the tin plate had been electrified positively by the condensing vapour.

Exp. III. A burning coal was placed upon the above-mentioned tin plate, and only two drops of water were let fall upon it. The tin plate was brought in contact with the plate A, and after very few additions the plate C electrified the electrometer .



ter to a degree much more than sufficient to shew that its electricity was positive, and that of course the tin plate had been electrified negatively by the evaporation of the two drops of water.

Exp. IV. The above-mentioned tin plate being held by the extremity of its insulating stand, I slapped it three times with my open hand. It was then made to touch the plate A of the multiplier.—After thirty additions, the electrometer received from the plate C a sufficiently sensible degree of positive electricity; shewing that the tin plate had been electrified negatively by the hand \*.

Exp. V. In order to examine the electricity of the atmosphere with the multiplier, I at first used to fix a long pointed wire into the socket of the plate A, and then exposed it to the open air. But I have lately used a much better method of

\* I have repeated this experiment several times with success; yet sometimes it has failed, perhaps owing to the various state of the skin of the hand, or of the surrounding air.

accomplishing

accomplishing that object. I expose, out of the window, an insulated stick of about five feet in length, and covered with tin foil; and whilst I hold this apparatus by the extremity of its insulating handle, I touch with the other hand, for about two or three seconds, the lower part of the stick. By this means the stick, being free from points, acquires an electricity contrary to that of the surrounding air. I then bring it within the room, and communicate that electricity to the plate A of the multiplier, &c. But the electricity so acquired by the insulated stick, is generally sufficiently strong to affect an electrometer without the use of the multiplier. To examine the electricity of the rain, snow, hail, &c. the same apparatus must be exposed out of a window, but the stick must not be touched, for in this case it acquires the same sort of electricity as that of the rain, snow, &c. and not the contrary sort, as when exposed to the air.

EXPERIMENTS

ON

METALLIC SUBSTANCES.

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**A**FTER many fruitless attempts, and after having sent to the press the preceding part of this volume, I at last hit upon a method of producing electricity by the action of metallic substances upon one another, and apparently without the interference of electric bodies. I say apparently so, because the air seems to be in a great measure concerned in those experiments, and perhaps the whole effect may be produced by that surrounding medium. But, though the irregular, contradictory, and unaccountable effects observed in these experiments do not as yet furnish any satisfactory theory, and though much is to be attributed to the circumambient air, yet the metallic substances themselves seem to be endowed

dowed with properties peculiar to each of them, and it is principally in consequence of those properties that the produced electricity is sometimes positive, at other times negative, and various in its intensity.

The discovery of those properties of metallic bodies opens a new field of useful investigation, and renders more manifest the general or extensive influence of a fluid wonderful in its nature and action. But how far they will enable us to explain the phenomena of Animal Electricity, and of other operations of nature, are considerations which will be noticed after the recital of the experiments. — In this account I shall endeavour to select and methodise the experiments, in the best manner that the irregularity of their results seems to admit of.

Exp. I. A piece of zinc, which weighed little more than half an ounce, was dropped ten times successively upon an insulated tin plate. This plate was then brought in contact with the plate A of the multiplier: the lever was worked, and after ten additions

tions of electricity, the plate C communicated to the electrometer a sufficiently sensible quantity of positive electricity, which shews that the tin plate had been electrified negatively by the contact of the zinc. This experiment was repeated four times within the space of half an hour, and was constantly attended with the like effect; but on the following day the effect was found to be less conspicuous, for three times twenty additions just enabled the plate C to communicate a sensible degree of positive electricity to the electrometer. In short, the different states of the atmosphere seem to be much concerned in the result of this experiment, and yet the whole effect cannot be attributed to it; but of this farther on. Before however I proceed to the narration of other experiments, it will be necessary to dwell a little longer on the above-mentioned operation, not only to render it more intelligible, but likewise to avoid repetitions.

The tin plate used in the preceding as well as in many of the subsequent experiments, measures eight inches in diameter;



and is fastened to a small piece of wood about three inches in length. Two glass sticks covered with sealing-wax are cemented into this piece of wood, and their other extremities are cemented into a larger piece of wood, which forms the stand or basis of the instrument. The operation is as follows: I hold this apparatus by the last-mentioned piece of wood in my left hand, and keeping the plate in an horizontal situation, let the piece of zinc or other metallic body, fall upon it from my right hand, which I hold a few inches above the plate; then by inclining or shaking the plate, the piece of metal is caused to fall upon the table or upon a chair; from whence I take it up, and again let it fall upon the tin plate, and so on.—During this operation, the tin plate must not be touched by any thing else besides the piece of metal which is under examination. As the glass sticks which support the tin plate, are never touched, there is no possibility of inducing any other electricity upon it, besides that which is produced by the contact of the other metal; but to avoid suspicion, whenever I am in doubt of the result of any experiment,



periment, I always try the tin plate before I repeat that experiment, viz. apply it to the plate A of the multiplier, then work the lever about thirty times, and after this if I find that the plate C of the instrument communicates no electricity to the electrometer, which is generally the case, I conclude that the plate is fit for the experiment.

Exp. II. A piece of zinc that weighed about six ounces, was dropped ten times successively upon the tin plate. The plate was then applied to the multiplier in the usual manner, and after ten additions the electrometer was electrified positively as in the first experiment, but to a greater degree.

Exp. III. The preceding experiment was repeated with only this variation, viz. that the zinc was now heated to about  $110^{\circ}$ . —The result was a higher degree of electrization; for after twenty additions the plate C caused the pendulums of the electrometer to touch the glass vessel in which

they were contained :—The electricity was positive.

I repeated the two last experiments several times, using the large piece of zinc alternately hot and cold ; but the effects though not precisely alike, yet they differed only in degree.

Exp. IV. A shilling was dropped ten times upon the tin plate, which was afterwards applied to the multiplier, and after ten additions the electrometer was made to diverge with positive electricity. In this experiment the divergency was not very great, but quite sufficient to shew that the electricity was positive.

This experiment being repeated several times, was attended with various events, the electricity being sometimes greater than at others, and once or twice it gave no electricity at all.

Exp. V. The preceding experiment was repeated with a half crown ; but after fifty  
additions

additions the electrometer was caused to diverge very little with positive electricity.

Exp. VI. The half crown was made very hot, and was then let fall as before ten times upon the tin plate, &c. but the success was not dissimilar from that of the preceding experiment.

Exp. VII. A new guinea was dropped ten times upon the tin plate, after which the tin plate was applied to the plate A of the multiplier, &c. After twenty additions the electrometer acquired a considerable degree of positive electricity.

Exp. VIII. The guinea was heated to about  $130^{\circ}$ , and was then treated exactly as in the preceding experiment. The electrometer received a small degree of positive electricity.

The two preceding experiments were repeated several times alternately, and the guinea was heated to different degrees; but from all those experiments it appeared, that the produced electricity was greater when

when the guinea was cold, and less when the guinea was hot. Indeed in the latter case it was sometimes next to nothing, so that it could be hardly manifested by three or four repetitions of about twenty additions.

- Exp. IX. A piece of copper, being a new halfpenny, was dropped ten times upon the tin plate; this plate was then applied to the receiving plate A of the multiplier, and the lever was worked in the usual manner. After twenty additions the electrometer was electrified positively by the plate C. The divergency of the pendulums was about an eighth of an inch.

Exp. X. The preceding experiment was repeated with this difference only, viz. that the piece of copper was first heated to about  $130^{\circ}$ .—The electrometer acquired a smaller degree of positive electricity.

Exp. XI. A piece of malleable platina that weighed about three ounces, was dropped ten times upon the above-mentioned tin plate, &c. After three times ten additions  
the

the electrometer shewed no divergency whatever; I therefore repeated the experiment several times, examining every part of the apparatus, and using every precaution that I could think of; but the result was, that the electrometer was either not at all, or very slightly, electrified; but when it was electrified I could plainly perceive that its electricity was positive.

Exp. XII. The platina was heated to about  $130^{\circ}$ , and in that state was dropped ten times upon the tin plate, &c.—After twenty additions the electrometer diverged more than half an inch with negative electricity. This being an unexpected effect, I was naturally induced to repeat the experiment, not only under the same, but likewise under some variety of circumstances. The general result was, that when the platina was used cold, viz. of the temperature of the air of the room, the electrometer acquired either no electricity at all, or a very small degree of positive electricity; but when the experiment was performed with the platina heated to above  $100^{\circ}$ , the electrometer acquired the negative electricity;

which, upon the whole, was stronger when the platina was heated to a greater degree. Sometimes the electricity was so strong, that after twice twenty additions the electrometer would strike against the sides of the glass vessel.

Exp. XIII. A piece of lead, that weighed somewhat more than a pound, was dropped ten times upon the tin plate. After twenty additions, the pendulums of the electrometer diverged to about a quarter of an inch with positive electricity.

Exp. XIV. The preceding experiment was repeated with this variation, viz. that the lead was heated to about  $130^{\circ}$ . The electricity produced was very little, but negative; the pendulums of the electrometer diverging about one-tenth of an inch after three times twenty additions.

Exp. XV. The same experiment was repeated with the lead made hotter, and after twice twenty additions the electrometer diverged upwards of half an inch with negative electricity.

Exp. XVI.



Exp. XVI. The same experiment was repeated with the lead made still hotter, but the electricity obtained, though negative as before, was however much less than in the preceding experiment; so that there seems to be a maximum in the degree of heat required, for the production of the greatest quantity of negative electricity.—This limit is observable with some other metallic substances as well as with lead.

Exp. XVII. A piece of iron was dropped ten times upon the usual tin plate, which was afterwards applied to the receiving plate of the multiplier, &c. The same piece of iron was also used in different degrees of heat; instead of this small piece a much larger lump of cast iron was tried in the like manner; but the result of those experiments was very equivocal, the electricity acquired by the electrometer, after twice or three times twenty additions, being always very weak, sometimes negative, at other times positive, and often not perceptible. It seemed to follow no certain law.

Exp. XVIII.

Exp. XVIII. A piece of grain tin, that weighed about four ounces, was dropped ten times upon the tin plate, &c. After twice twenty additions the pendulums of the electrometer diverged about a quarter of an inch with positive electricity.

Exp. XIX. The same piece of tin was heated to about  $130^{\circ}$ , and was then treated exactly as in the preceding experiment.—The electricity obtained was stronger but still positive. In other subsequent trials this increase of electricity kept pace with the increase of heat; but when the tin was heated so as to be not far from a melting heat, the electricity though of the same quality, was however less in quantity than in lower temperatures.

Exp. XX. The tin being cooled down to the temperature of the air, I repeated the 18th Experiment with this variation, viz. that instead of the hand as used in the preceding trials, the tin was dropped upon the plate from a pair of iron tongs, and after it had been let fall from the tin plate upon a chair, it was from thence picked up again  
by

by means of the same iron tongs which I held in my right hand. After twice twenty additions the electrometer diverged very little but with negative electricity.

Exp. XXI. The preceding experiment was repeated with the tin made hot, and the electricity obtained was of the same quality but greater in quantity. The remarkable difference produced by the tin being dropped from the hand or from the iron tongs; the former electrifying the electrometer positively, and the latter negatively, naturally induced me to repeat these singular experiments several times, using alternately the iron tongs or the immediate application of the hand, but the results were generally alike; I say generally, because it happened two or three times, amongst twenty experiments and upwards, that the electricity obtained was different from what I expected. Whether those unusual effects originated from some oversight in the operation, or are to be attributed to some other circumstance, I cannot possibly pretend to determine. Certain it is, that the electricity of the surrounding air, and per-  
haps

haps some other causes, frequently concur to alter the effect, or intirely to change the electricity from positive to negative, or from the latter to the former, which will be more evidently shewn by the following experiments and observations.

Exp. XXII. A piece of bismuth, weighing about an ounce, was dropped in the usual manner ten times upon the tin plate. This plate was then applied to the receiving plate of the multiplier, and after twenty additions the electrometer diverged with negative electricity.

Exp. XXIII. The preceding experiment was repeated several times over, with the piece of bismuth made successively hotter and hotter. The result was, that the quantity of electricity gradually became smaller and smaller, and at last it became positive, but its quantity was then very small indeed; for after three times twenty additions, the pendulums of the electrometer diverged about one-tenth of an inch. It was however sufficient to shew, that in bismuth the electrical phenomena follow an  
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order different from that of the generality of metallic substances; for with several of them the heat seems to diminish the positive electricity, and at last to change it to negative, whereas with bismuth heat changes it from negative to positive.

The peculiar properties of bismuth, as shewn by the two preceding experiments, naturally suggested the necessity of examining very particularly into the concurring circumstances, in order to discover whether the peculiar effects produced by the experiments on bismuth, were really owing to the nature of that semimetal or to some accidental circumstance. The first suspicion fell upon the shape of it; for the piece, which I had used, was a fragment from a larger piece. It had been broke off by a stroke of the hammer, and was of course very irregular, and full of sharp edges and points, some of which frequently broke off in the act of dropping the piece upon the tin plate. To put therefore this possible source of error out of the question, I fused a quantity of bismuth in a crucible, and by pouring part of it upon a marble slab,

slab, I obtained a smooth and flat piece as like as possible to a piece of tin, which I had already used in these experiments, and which, I knew, would electrify the electrometer positively when treated in the above-described manner. With those two pieces I repeated the experiment a great number of times, alternately using the tin and the bismuth, and took care to perform every other operation exactly in the same manner; but when the piece of tin was used, the electricity acquired by the electrometer was positive, and when the bismuth was tried, the electricity was negative, excepting when either of the two metallic pieces was too much heated. These experiments therefore seem to prove, beyond the possibility of doubt, that there is something in the very nature of a metallic body, that disposes it to part with or to attract the electric fluid more easily in certain circumstances, than other substances of the same class.

One of the powers which I have hitherto found capable of altering or changing that tendency of metallic bodies, to part with  
or



or attract the electric fluid, is the action of heat. Had that action produced the same change in them all, an hypothesis might perhaps have been formed on the principle of rarefaction and condensation; but we find the case to be otherwise.

Exp. XXIV. I had the curiosity to try whether the application of the iron tongs to the bismuth, was attended with any peculiar phenomenon, and for this purpose I dropped the piece of bismuth from the tongs upon the tin plate, exactly as I had done with the tin in Experiment the 20th. The result was, that the electricity imparted to the electrometer was now positive, whereas when the hand was immediately applied to the bismuth, the electrometer was electrified negatively. I own that I am at a loss to account for those phenomena. It is however evident, that different metallic bodies have a different affinity, or attraction, towards the electric fluid; and that in consequence of this disposition, (which seems to be inherent in their natures, and not merely owing to external configuration.

configuration) their contact with one another, or with the air, is sufficient to disturb the electric equilibrium.

Hitherto the metallic bodies had been dropped successively upon the same substance, viz. the tin plate; and by this means I endeavoured to form a table of their respective properties; but the great difference observed in the results of similar trials repeated at different times, and especially under different states of the atmosphere, proved, first, that great accuracy in the determination of those properties was not to be expected; and, secondly, that the phenomena observed in these experiments are in a great measure to be attributed to the surrounding air, or to the disposition of the weather. In order to carry on this investigation, my next object was to drop the metallic bodies upon some other insulated metal, and for this purpose I fastened a large silver spoon upon an insulating stand, like that of the tin plate used in the foregoing experiments.

Exp. XXV.

Exp. XXV. Into this spoon I dropped a piece of zinc from my hand ten times repeatedly; then presented the spoon to the receiving plate of the multiplier, and worked the lever in the usual manner. After twenty additions the electrometer was electrified positively by the plate C; its pendulums diverging about half an inch. This experiment having been repeated five or six times in the same hour, was always attended with the like effect.

Exp. XXVI. The preceding experiment was repeated with the zinc heated to about  $110^{\circ}$ . The electricity obtained after twenty additions, was positive but stronger than before.

Exp. XXVII. The same experiment was tried again with the zinc heated to a higher degree; and after a similar operation, the electricity obtained was so strong that the pendulums of the electrometer diverged full one inch.

Exp. XXVIII. In order to carry this experiment still farther, I cooled the spoon

out of the window, and made the piece of zinc so hot, that I could with difficulty handle it. The rest of the experiment was performed in the same manner as before. After twenty additions, the pendulums of the electrometer struck the sides of the glass vessel. The electricity was positive as before.

Exp. XXIX. The temperatures of the two metallic bodies were reversed, viz. the spoon was heated, and the zinc was cooled out of the window; the other operations were performed in the usual manner. The electricity obtained was positive, but so weak that, after twice twenty additions, the pendulums of the electrometer hardly diverged one-eighth of an inch. On the same day I repeated this experiment over and over again; sometimes heating the spoon, and at other times heating the zinc to various degrees of heat. The event upon the whole was, that the electricity imparted to the electrometer was always positive. It was strongest when the silver spoon was cold and the zinc hot; but it decreased gradually as they came nearer to  
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the same temperature, and followed the same decrease beyond that limit, viz. when the silver spoon was used hot and the zinc cold. But it is very extraordinary, and indeed it surprized me a good deal to find, on repeating the same experiments on two succeeding days, that the electricity obtained was so very weak, that it could be just observed when the spoon was of the usual temperature of the atmosphere, and the zinc was heated a little. Every precaution I could think of, was found insufficient to produce the same phenomena, as I had observed in the 25th, 26th, 27th, 28th, and 29th Experiments. The only difference I could remark was in the disposition of the atmosphere; on the first day the wind being easterly, and the air very clear and dry; whereas on the other days the weather was rainy, and the wind was westerly.

Most of those experiments have been repeated since in different states of the weather, and with some variety in the operation, but no determinate law could be discovered; the electricity obtained being

sometimes strong, and at other times so weak, as to be manifested with difficulty. The state of the atmosphere is certainly concerned in it, the electricity being generally, though not always, stronger in dry and cold weather. In that state of the weather I leave the silver spoon or tin plate in its usual temperature, heat a piece of zinc or tin to about  $110^{\circ}$  or  $120^{\circ}$ , throw it from my hand into the spoon about a dozen of times, then apply the spoon to the receiving plate of the multiplier, and after twice twenty additions, I almost always obtain some positive electricity, which shews that the spoon is electrified negatively by the contact of the zinc or tin. When the experiment is repeated exactly in the same manner, but with bismuth instead of zinc or tin, the electricity obtained is negative, shewing that the spoon is electrified positively by the contact of the bismuth, especially when the bismuth is cold; but if it be heated to about  $200^{\circ}$  and upwards, the electrometer will be electrified positively; it seems therefore evident, that the production of different electricities is owing to the different natures of the metallic substances;

for



for whether the electricity be derived from the air, from the hand, or merely from the contact of the two metals, its different quality cannot depend upon any other circumstance than the particular nature of the metal employed, that being the only thing varied in the experiment.

Exp. XXX. One day, when these experiments succeeded remarkably well, I was desirous of observing whether by dropping the zinc upon the spoon a different number of times, viz. by dropping it once or twice, or any other number of times, the electricity thereby obtained kept increasing in the same proportion. Upon the whole, I found that when the zinc was dropped about thirty times, the electricity produced seemed to be at the maximum; but from ten or fifteen to thirty, the difference is hardly perceptible. When the zinc is dropped four or six times, the electricity is certainly not so considerable as when it is dropped more than ten times.

Exp. XXXI. In order to discover the source of the electricity produced in these

experiments, I tried the experiment in a great variety of ways, viz. instead of the hand, I dropped the zinc from a tin plate held with one hand, into the spoon; and from the latter back upon the former. I performed this operation when both the tin plate and the silver spoon were insulated; I likewise tied a silk thread to the zinc, and holding the other extremity of the thread in one hand, struck the zinc repeatedly against the spoon; but in those cases very seldom any electricity was manifested, except when the weather and every other circumstance was very favourable, and then the electricity could with difficulty be manifested. However, when the tin plate is held in the hand, and the zinc is thrown from it into the insulated spoon, some electricity is more frequently produced than in the other two cases.

After a careful review of the foregoing experiments, I still doubt whether the phenomena of Animal Electricity may be attributed to the electricity generated by the contact of metallic bodies. Had the action of the same metals upon prepared ani-

mal bodies produced effects as fluctuating as those of the preceding experiments, we might without hesitation have admitted the proposition; but when we find that in different states of the atmosphere, the action of metallic bodies produces the contractions of prepared animal limbs, with hardly any observable difference; and when we consider that zinc and bismuth, which though in the above-mentioned experiments do more generally produce different states of electricity, yet they are not more powerful exciters of Animal Electricity, than zinc and silver, or zinc and gold; we can then hardly conclude with saying, that the effects observed in Animal Electricity are to be intirely attributed to the electricity produced by the contact of the metallic bodies.

It may indeed be said, that if the mutual contact of metals amongst themselves produces a sensible quantity of electricity in most cases, it is likely that they always produce some electricity, which may be too little to be rendered manifest by the multiplier and an electrometer, but may yet be

sufficient to produce the contraction of animal muscles. I must however observe, that the quantity of electricity, which is necessary to contract a prepared animal limb, does not seem to be so small as that which is produced when two small pieces of metal come to touch each other. When a Leyden phial is used in experiments on prepared animals, the quantity of electricity employed cannot be properly estimated by means of an electrometer. Thus a Leyden phial, after having been charged and discharged, may contain a residuum, which is absolutely incapable of affecting an electrometer; yet when condensed upon a small piece of metal, may be sufficient not only to affect an electrometer, but to produce even a spark. In order therefore to compare, in a less equivocal manner, the quantity of electricity which can affect an electrometer, with that which is sufficient to contract the prepared animals, I electrified an electrometer until its pendulums diverged to about a twentieth of an inch, then touched the electrometer with an insulated piece of metal, the surface of which was about 200 times greater than that of the  
conducting

conducting parts of the electrometer, and withdrew it immediately after. By this means it is evident, that the electrometer was left with about the 200th part of that electricity, which had been at first communicated to it. On touching the prepared legs of a frog with this electrometer, possessed of that small quantity of electricity, no contraction ensued. I have repeated this experiment many times, and have found that the motions are very seldom excited by the passage of so small a quantity of electricity; yet I am sure, that in the experiments of touching one metal with another, the electricity produced is very frequently much less than that which was possessed by the electrometer just mentioned. Nevertheless, if a prepared animal be touched with those metals, the convulsions are immediately excited. Are we then not authorized to say, that either the quantity of electricity which is produced in the act of touching animals with metallic substances, is greater than that which is produced by the contact of one metal with another metal; or that the contractions excited in animal bodies by the application of metals, must be attributed to

to some other property of metallic substances, independent of electricity? — It seems, however, proper to suspend for the present any hasty conclusion, which may mislead the inquirers into this new subject, and to wait for farther discoveries, and more decisive experiments. I shall therefore conclude this section, with a short enumeration of the particulars which the preceding experiments seem to establish.

1. The contact of one metallic substance with another generally produces electricity.

2. The quantity and quality of the electricity so produced, is various according to many circumstances which seem to concur in the production of it, or in great measure to influence it.

3. Those circumstances are, the various nature of the metallic substances, their various degrees of heat, the state of the atmosphere, and the other body concerned in the experiment, viz. the hand of the operator, &c. Each of those causes has a share



in the result of the experiment; for the variations of any one of them, when every thing else remains unaltered, produce different effects. Thus in different states of the atmosphere, the very same metallic substances treated exactly in the same manner, produce a greater or less quantity of electricity. Thus also, by only heating or cooling the metals, the electricity may be varied in quantity and even in quality.

I am inclined to suspect, that different bodies have different capacities for holding the electric fluid, as they have for holding the elementary heat; if however the experiments relative to this subject be carefully tried, under all the variety of circumstances which the combination of the above-mentioned causes is capable of producing, I do not doubt but that all the phenomena observed in the preceding pages may hereafter be reconciled to one, or to a few, simple laws, which will at the same time assist the farther investigation of the science of electricity.

## DESCRIPTION of the METHODS

Of producing diverse curious CONFIGURATIONS by MEANS of

## ELECTRICITY.

IN the seventh chapter of the fourth part of this treatise, mention is made of some configurations described by Professor LICHTENBERG, of Gottingen, who first observed them upon a large electrophorus; and in the same chapter several experiments are related, which were made in explanation of those phenomena. Since the original discovery of those configurations by Professor LICHTENBERG, diverse methods have been contrived of producing them, and of fixing them upon paper, silk, resinous substances, &c. Their various shapes and ramifications afford a good deal of information to the speculative electrician, by shewing in many cases the direction, division, and quality

§

lity

lity of the electricity in a convincing and permanent manner. But independent of this scientific instruction, some of those impressions are very beautiful ornaments, and as such, it is not unlikely, that they may hereafter be introduced in various manufactures.

The principal method of producing those impressions is, in general, to electrify a perfect or imperfect electric, and then to throw certain powders upon it, which will dispose their particles into various forms. Those powders are projected different ways, which, to avoid repetition, will be here described previous to the particular methods of forming those impressions. The powders may be sifted over the electrified body from a common sieve; they may be tied up in linen rags and shook out of them; they may be projected by means of a brush\*, and by means of a pair of bellows; but a more

\* This may be done by taking a little of the powder between a finger and thumb, and drawing it over the brush, or by rubbing upon the brush a lump of some substance that is easily reduced into powder, such as chalk, whiting, &c.

convenient method is the following: To the neck of a small bottle of elastic gum, which is more commonly called *India rubber*, tie a tube of glass, wood, or metal; put the powders which you want to project, into this bottle, and then tie a double piece of flannel over the aperture of the tube. If this bottle so prepared be held in the hand, and be squeezed, by alternately opening and shutting the hand, the powders will be projected in a fine diffused manner.

As for the nature of the powders, almost every substance that can be pulverized sufficiently fine, will produce some configurations when projected upon an electrified substance. Thus chalk, sulphur, cinnabar, rosin, dragon's blood, gum arabic, evaporated decoctions of colouring woods, and many others, may be employed for this purpose either single or mixed.

Exp. I. Take a pane of glass clean and dry, hold it suspended by one corner, or lay it flat upon a table, and draw over the surface of it the knob of a Leyden phial, moderately

derately charged with positive electricity. Then lift up the glass, if laid upon a table, and, holding it suspended, project upon it, by means of the elastic gum bottle, a mixed powder consisting of dragon's blood and gum arabic in equal parts. You will find that the two powders will be separated upon the glass; the red powder of dragon's blood falling on certain places, and the whitish powder of gum arabic falling on certain other places, so as all together to form an oblong radiated track, consisting of two colours, intermixed in a thousand odd ways\*.

If instead of drawing the knob of the jar over the surface of the glass, you only touch the surface here and there with it, and then throw the mixed powder upon it as before; separate star-like figures will be formed about those points. The stars,

\* Having shewn in the 7th chap. of the 4th part, that in the act of projecting those powders some of them acquire the positive, and others the negative electricity, I need not repeat, in this place, the reasons why the dragon's blood and gum arabic take different places in this or in the following experiments.

however,

however, are better defined when a single powder is projected: their rays or ramifications are sometimes few and strong, at other times many and slight; and frequently they don't go quite round the points on which the knob of the jar has been touched \*. At A and B of fig. 6th, in the annexed plate, two of those sparks are delineated, in one of which the ramifications go quite round, but in the other they do not.

Exp. II. Repeat the preceding experiment with only this variation, viz. that now the Leyden phial be charged negatively, and the appearance of the configuration will be vastly different from that occasioned by positive electricity. In this very few rays or branches will be observed; the powders mostly disposing themselves in roundish spots, and generally a central spot of one powder will be surrounded by another powder of another colour.

\* These various effects depend principally on the greater or lesser quantity of electricity contained in the Leyden phial.

Instead



Instead of dragon's-blood and gum arabic, powders of other colours may be projected upon the pane of glass, such as powder of Prussian blue, sulphur, vermilion, rosin, &c.; and thus the colours of the configurations may be varied.

Those powders adhere to the glass rather slightly, viz. so as not to bear being touched; yet if a piece of paper be laid on the painted side of the glass without rubbing it, and the edge of it be glued all round the edge of the glass, the figures may be preserved without injury. But a better method is, to lay another pane of glass of the same size upon the glass with the configurations, and to fasten them round with sealing-wax, or a slip of paper pasted over both their edges.

If powders of such colours as are used for enamel-painting, be projected upon glass or porcelain, and these substances be afterwards exposed to a proper degree of heat in an enameller's furnace, the configurations will thereby be rendered indelible.

Exp. III. Take a piece of common writing paper, hold it very near the fire, so as to render it quite dry and very hot ; lay it flat upon a dry marble slab or a very dry table, and in that situation draw over it the knob of a charged Leyden phial, then lift up the piece of paper by one corner, and holding it suspended, project upon it the mixed powder of dragon's-blood and gum arabic by means of the elastic gum bottle. The configurations in this case are very beautiful, and may be made in various shapes, as of letters, stars, stripes, &c. by moving the knob of the Leyden phial in the desired direction ; but they are of one colour, viz. red ; for the gum arabic being nearly of the colour of the paper, cannot be distinguished upon it.

If the paper thus painted be held very near the fire for a few seconds of time, the powder of dragon's-blood, being a resinous substance, will be melted, and fastened to the paper ; after which the powder of gum arabic may be wiped off with a handkerchief.

Powders of other colours may be projected upon the paper after the same manner; but unless they are of a resinous nature, so as to be easily melted by heat, it is very difficult to fasten them to the paper.

A little experience will soon enable the operator to make those impressions in a neat and definite manner; nevertheless it will be proper, before we describe the other experiments, to mention some useful precautions. The charge of the Leyden phial for these experiments must be neither too high nor too low; for in the former case, the figure obtained will be too confused and irregular, and in the latter it will be too faint. In order to form a neat and well-determined figure, and to leave the rest of the paper clean, the powders must not be projected perpendicularly to the paper, but the stream must be thrown in a direction parallel to the surface of the paper. It is also necessary to perform these experiments in as quick a manner as possible, for if the paper be suffered to cool too much, or the communicated

electricity to dissipate, the desired effect will not be obtained.

Exp. IV. Instead of the paper, the impressions may be made upon marble, by drawing the knob of the charged jar upon it, in the same manner as it is done upon paper. For this purpose, the marble must be made dry and hot. After the same manner, the configurations may be made upon all sorts of electric or semi-electric substances, and they may be preserved by covering them with a pane of glass, by partially melting either the resinous plate or the powders, if they are of a proper nature; or, lastly, by transposing them from the electric plate upon a piece of paper. But this last method is attended with a good deal of difficulty. I shall, however, (for the sake of those persons who are not unwilling to undergo the trouble attending it) subjoin Mr. BENNET's method of performing it, which is very minutely described in his work on Electricity.

“To make red figures, take a pound of  
rasped Brazil wood, put it into a kettle

“ with as much water as will cover it, or  
 “ rather more ; also put in about an ounce  
 “ of gum arabic and a lump of allum about  
 “ as big as a large nut, let it boil about  
 “ two hours, or till the water is strongly  
 “ coloured ; strain off the extract into a  
 “ broad dish, and set it in an iron oven,  
 “ where it is to remain till all the water be  
 “ evaporated, which with me was effected  
 “ in about twelve hours ; but this depends  
 “ on the heat of the oven, which should  
 “ not be so hot as to endanger its burning.  
 “ Sometimes I have boiled the strained ex-  
 “ tract till it was considerably inspissated  
 “ before it was placed in the oven, that it  
 “ might be sooner dry.

“ When it is quite dry but not burnt,  
 “ scrape it out of the dish, and grind it in a  
 “ mortar till it be finely pulverized. In  
 “ doing this, it is proper to cover the mor-  
 “ tar with a cloth, having a hole through  
 “ to prevent the powder from flying away  
 “ and offending the nose, and also to do it  
 “ out of doors if the weather be dry and  
 “ calm, that the air may carry away the  
 “ powder necessarily escaping, and which

“ otherwise is very disagreeable. When  
 “ ground fine, let it be sifted through mus-  
 “ lin or a fine hair-sieve, returning the  
 “ coarser part into the mortar to be ground  
 “ again. When the grinding and sifting  
 “ are finished, the powder is ready for use.  
 “ The resinous plate I have mostly used  
 “ was composed of five pounds of rosin,  
 “ half a pound of bees-wax, and two  
 “ ounces of lamp-black, melted together,  
 “ and poured upon a board sixteen inches  
 “ square, with ribs upon the edges at least  
 “ half an inch high, to confine the compo-  
 “ sition whilst fluid; thus the resinous plate  
 “ was half an inch thick, which is better  
 “ than a thinner plate, the figures being  
 “ more distinct. After the composition is  
 “ cold, it will be found covered with small  
 “ blisters, which may be taken out by hold-  
 “ ing the plate before the fire, till the sur-  
 “ face be melted, then let it cool again, and  
 “ upon holding it a second time to the fire,  
 “ more blisters will appear; but by thus  
 “ repeatedly heating and cooling the sur-  
 “ face, it will at last become perfectly  
 “ smooth. Some plates were made smaller,  
 “ and the resinous composition confined to



“ the form of an ellipsis, a circle or escut-  
 “ cheon, by a rim of tin half an inch broad,  
 “ and fixed upon a board.

“ The next thing to be done is to prepare  
 “ the paper, which is to be softened in wa-  
 “ ter, either by laying the pieces upon each  
 “ other in a vessel of cold water, or first  
 “ pouring a little hot water upon the bot-  
 “ tom of a large dish, then laying upon it a  
 “ piece of paper, so that one edge of the  
 “ paper may lie over the edge of the dish,  
 “ to remain dry, that it may afterwards be  
 “ more conveniently taken up. Then pour  
 “ more hot water upon its upper surface.  
 “ Upon this place another piece in the same  
 “ manner, again pouring on more water,  
 “ and thus proceed till all the pieces are  
 “ laid in. By using hot water, the paper  
 “ will be more softened in a few minntes  
 “ than if it remains in cold water a whole  
 “ day.

“ When the figures are to be made, the  
 “ resinous plate must lie horizontally, whilst  
 “ the electricity is communicated, if the  
 “ experiment requires any thing to be placed  
 “ upon

“ upon the plate : but it is convenient af-  
“ terwards to hang it up in a vertical posi-  
“ tion whilst the powder is projected, lest  
“ too much powder should fall where it is  
“ not required.

“ A little of the powder may be taken  
“ between a finger and thumb, and pro-  
“ jected by drawing it over a brush, or,  
“ which is better, a quantity of powder  
“ may be put into the bellows and blown  
“ towards the plate. When the figure is  
“ sufficiently covered with powder, let the  
“ plate be again laid horizontally upon a  
“ table, then take one of the softened pa-  
“ pers out of the water by its dry edge,  
“ and lay it carefully between the leaves of  
“ a book, pressing the book together, and  
“ let it lie in this situation about half a  
“ minute. Then remove the paper to a  
“ dry place in the book, and press it again  
“ about the same time, which will generally  
“ be sufficient to take off the superfluous  
“ moisture. Then take up the paper by  
“ the two corners of its dry edge, and place  
“ the wet edge a little beyond the figure on  
“ the resinous plate, lowering the rest of  
“ the

“ the piece gradually till it covers the figure  
 “ without sliding, then lay over it a piece  
 “ of clean dry paper, and press it gently,  
 “ let it remain a short time, and then rub  
 “ it closer to the plate with a cloth, or,  
 “ which is better, press it down by means  
 “ of a wooden roller covered with cloth,  
 “ taking care that the paper be not moved  
 “ from its first position. When the paper  
 “ is sufficiently pressed, let it be taken up  
 “ by its dry edge, and laid upon the surface  
 “ of a vessel of water with the printed side  
 “ downwards, by this means the super-  
 “ fluous powder will sink in the water, and  
 “ the figure will not be so liable afterwards  
 “ to spread in the paper. After the paper  
 “ has remained on the water during a few  
 “ minutes, take it up and place it between  
 “ the leaves of a book, removing it fre-  
 “ quently to a dry place. If it be desired  
 “ that the paper should be speedily dry, let  
 “ the book-leaves in which it is to be  
 “ placed be previously warmed, and by re-  
 “ moving it to several places it will be dry  
 “ much sooner than by holding it near a  
 “ fire, and without drawing the paper  
 “ crooked. By the above process, it is ob-  
 “ vious,

“ vious, that leather, callico, or linen, as  
 “ well as paper, may be printed with these  
 “ figures, and the effects of the diffusion  
 “ of electricity upon a resinous plate be  
 “ exhibited to those who have not leisure  
 “ or inclination to perform the experi-  
 “ ments.”

Exp. V. A very regular figure may be formed on the surface of a resinous plate by the following means: Lay the resinous plate upon a table; insulate one or more pointed wires over the plate, with their points directed towards its surface, and at about the distance of an inch and a half or two inches. Then by touching those wires alternately with a positive and a negative Leyden phial, throw the alternate sparks upon the resinous surface; and afterwards, by projecting different coloured powders upon the plate, you will obtain very regular figures, consisting of concentric zones of different colours, which assume the form of circles, ellipses, or other curves, according as one or more pointed wires are used, and according as those wires are placed nearer or farther from each other.

Exp.

Exp. VI. Cut a figure of any sort out of the middle of a card, as for instance a profile, a flower, &c. Place a piece of white silk (white sattin answers very well) upon a table; lay the card upon it, and a gold leaf over the card; in which situation it is evident that the gold leaf will touch the silk only within the limits of the figure that has been cut out of the card. This done, lay another card over the gold leaf, and put a book, or something else heavy, upon it to keep it down. Care, however, must be taken to leave two projections of the gold leaf out of the card at opposite ends. Lastly, if you send the charge of a battery through the gold leaf, by connecting one of its projections with the inside, and the other with the outside of the battery, the gold will be melted and forced into the substance of the silk, so as to stain it with a purple spot of the shape and size of the figure cut in the card. The battery for this experiment must have force sufficient to melt the gold leaf completely. I need hardly observe, that by changing the metallic leaf, as well as the figure in the card,

card, many beautiful ornaments may, by this means, be marked upon silk.

Exp. VII. Hold a piece of writing paper near the fire to render it dry and warm: then lay it upon a table, and rub it with a dry hand, which operation will electrify the paper. Now light a piece of sealing-wax with a candle, and after having suffered it to burn for about five or six seconds of time, lift up the excited paper from the table, and hold it up by one corner; blow out the flame of the sealing-wax, and present the melted end of it to the paper at the distance of about an inch, moving it in various directions very quickly. In doing this, the electricity of the paper will attract the sealing-wax into the shape of exceedingly fine filaments, which may afterwards be melted and fastened to the paper, by holding the paper very near the fire for a short time. A small piece of sealing-wax stuck upon a wire or a pin, answers better than a common flick of sealing-wax. The impressions made in this manner, are in general not so beautiful as those described in the preceding pages; yet this experiment



ment is attended with a considerable advantage, which is, that it does not require the electrical machine, or other apparatus, and may of course be performed in any place.

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OF THE  
EFFECTS produced by ELECTRICITY  
On permanently ELASTIC FLUIDS, and on WATER.

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**I**N the preceding volumes of this treatise, notice has been taken of some effects produced by Electricity on certain permanently elastic fluids, and likewise upon water; but since the third edition of my treatise, some capital discoveries have been made relative to this subject, which, besides their being remarkably curious in themselves, are likely to unfold some general operations in the grand laboratory of Nature.

With respect to the permanently elastic fluids, it has been discovered, that by the action of Electricity a mixture of dephlogisticated and common air, or of dephlogisticated

ticated and phlogistified air, is converted into nitrous acid; and with respect to water it has been found, that by means of electric shocks, that fluid is converted into an inflammable and permanently elastic fluid, resembling a mixture of inflammable and common air, which by inflammation is again converted into water. The first of those discoveries is due to the genius of Henry Cavendish, Esq; F. R. S. and an accurate description of his experiments, &c. was published in the 75th and 78th volumes of the Phil. Transactions, from which the following account has been extracted mostly in his own words.

The quantity of air through which the electric sparks were required to be passed, was confined in the angular part of a glass tube, bent to an angle, and inverted with its two open extremities in two glasses nearly full of quicksilver; the remaining cavity of the glass tube being likewise filled with quicksilver \*. In order to force the electric

\* The bore of the tube mostly used for those experiments, was about one-tenth of an inch in diameter; and the

tric spark through the tube, a metallic ball was insulated at a small distance of the prime conductor of an electrical machine, from which it received the sparks; and a communication was made between the ball and the quicksilver in one of the glasses, while the quicksilver of the other glass communicated with the ground. In these experiments the electrization must be continued for a considerable time. If the electrical machine works well, its prime conductor giving between two and three hundred sparks in a minute to the ball, and if the machine be worked for about half an hour a day, the experiment will hardly be completed in a fortnight or three weeks time.

“ When the electric spark was made to  
 “ pass through common air, included be-

the length of the column of air occupying the upper, and middle part of the tube, was in general from  $1\frac{1}{2}$  to  $\frac{3}{4}$  of an inch. The tube was first entirely filled with quicksilver, and was inverted with its extremities into the quicksilver contained in the two glasses; then the proper quantity of air was introduced in it by means of a well-contrived apparatus, which is minutely described in the 75th vol. of the Phil. Transf. p. 372, and following.

“ tween

“ tween short columns of a solution of  
 “ litmus, the solution acquired a red co-  
 “ lour, and the air was diminished con-  
 “ formably to what was observed by Dr.  
 “ Priestley.

“ When lime-water was used instead of  
 “ the solution of litmus, and the spark was  
 “ continued till the air could be no further  
 “ diminished, not the least cloud could be  
 “ perceived in the lime-water ; but the air  
 “ was reduced to two-thirds of its original  
 “ bulk, which is a greater diminution than  
 “ it could have suffered by mere phlogisti-  
 “ cation, as that is very little more than one-  
 “ fifth of the whole.

“ The experiment was next repeated  
 “ with some impure dephlogisticated air ;  
 “ the air was very much diminished, but  
 “ without the least cloud being produced  
 “ in the lime - water. Neither was any  
 “ cloud produced when fixed air was let up  
 “ to it ; but on the further addition of a  
 “ little caustic volatile alkali, a brown sedi-  
 “ ment was immediately perceived.

“ Hence we may conclude, that the  
 “ lime-water was saturated by some acid  
 “ formed during the operation; as in this  
 “ case it is evident, that no earth could be  
 “ precipitated by the fixed air alone, but  
 “ that caustic volatile alkali, on being add-  
 “ ed, would absorb the fixed air, and thus  
 “ becoming mild, would immediately pre-  
 “ cipitate the earth; whereas, if the earth  
 “ in the lime-water had not been saturated  
 “ with an acid, it would have been precipi-  
 “ tated by the fixed air. As to the brown  
 “ colour of the sediment, it most likely pro-  
 “ ceeded from some of the quicksilver hav-  
 “ ing been dissolved.”

Mr. Cavendish made several experiments  
 with a view to determine what degree of  
 purity the air should be of in order to be  
 diminished most readily, and to the greatest  
 degree; and found, that good dephlogisti-  
 cated air was diminished very little; per-  
 fectly phlogisticated air was not sensibly di-  
 minished: but a mixture of dephlogisticated  
 and phlogisticated air in the proportion of  
 five parts of the former to three parts of  
 the



the latter, was made almost entirely to disappear.

“ Having made, *says he*, these previous  
 “ trials, I introduced into the tube a little  
 “ soap-lees, and then let up some dephlo-  
 “ gisticated and common air, mixed in the  
 “ above-mentioned proportions, which ris-  
 “ ing to the top of the tube, divided the  
 “ soap-lees into its two legs. As fast as  
 “ the air was diminished by the electric  
 “ spark, I continued adding more of the  
 “ same kind, till no further diminution took  
 “ place: after which a little pure dephlo-  
 “ gisticated air, and after that a little com-  
 “ mon air were added, in order to see whether  
 “ the cessation of diminution was not ow-  
 “ ing to some imperfection in the propor-  
 “ tion of the two kinds of air to each other,  
 “ but without effect \*. The soap-lees  
 “ being

\* “ From what follows it appears, that the reason why  
 “ the air ceased to diminish was, that as the soap-lees  
 “ were then become neutralized, no alkali remained to  
 “ absorb the acid formed by the operation, and in conse-  
 “ quence scarce any air was turned into acid. The  
 “ spark, however, was not continued long enough after  
 “ the apparent cessation of diminution, to determine with

“ being then poured out of the tube, and  
 “ separated from the quicksilver, seemed to  
 “ be perfectly neutralized, as they did not  
 “ at all discolour paper tinged with the  
 “ juice of blue flowers. Being evaporated  
 “ to dryness, they left a small quantity of  
 “ salt, which was evidently nitre, as appear-  
 “ ed by the manner in which paper, im-  
 “ pregnated with a solution of it, burned.

“ For more satisfaction, I tried this ex-  
 “ periment over again on a larger scale.  
 “ About five times the former quantity of  
 “ soap- lees were now let up into a tube of  
 “ a larger bore ; and a mixture of dephlo-  
 “ gisticated and common air, in the same  
 “ proportions as before, being introduced,  
 “ the spark was continued till no more air  
 “ could be made to disappear. The liquor,  
 “ when poured out of the tube, smelled  
 “ evidently of phlogisticated nitrous acid,  
 “ and being evaporated to dryness, yield-

“ certainty, whether it was only that the diminution went  
 “ on remarkably slower than before, or that it was al-  
 “ most come to a stand, and could not have been carried  
 “ much further, though I had persisted in passing the  
 “ spark,”

“ ed

“ ed  $1\frac{4}{10}$  gr. of salt, which is pretty exactly  
 “ equal in weight to the nitre which that  
 “ quantity of soap-lees would have afforded,  
 “ if saturated with nitrous acid. This salt  
 “ was found, by the manner in which pa-  
 “ per dipped into a solution of it burned, to  
 “ be true nitre. It appeared by the test of  
 “ *terra ponderosa solita*, to contain not more  
 “ vitriolic acid than the soap-lees them-  
 “ selves contained, which was excessively  
 “ little, and there is no reason to think that  
 “ any other acid entered into it, except the  
 “ nitrous.”

The dephlogisticated air used in the fore-  
 going experiments was not extracted from  
 nitre, but from the black powder formed  
 by the agitation of quicksilver mixed with  
 lead, or from turbith mineral. “ In the  
 “ first experiment, the quantity of soap-  
 “ lees used was 35 measures, each of which  
 “ was equal in bulk to one grain of quick-  
 “ silver; and that of the air absorbed was  
 “ 416 such measures of phlogisticated, and  
 “ 914 of dephlogisticated. In the second  
 “ experiment, 178 measures of soap-lees  
 “ were used, and they absorbed 1920 of

“ phlogisticated air, and 4860 of dephlo-  
 “ gisticated. It must be observed, how-  
 “ ever, that in both experiments some air  
 “ remained in the tube uncondensed, whose  
 “ degree of purity I had no way of trying ;  
 “ so that the proportion of each species of  
 “ air absorbed is not known with much ex-  
 “ actness.”

Some time after the publication of the  
 above-mentioned discovery, Mr. Cavendish,  
 in order to remove some difficulties which  
 had prevented the success of other persons,  
 who had attempted to repeat his experi-  
 ments, and likewise to confirm his former  
 assertions, caused his principal experiments  
 to be repeated by a person versed in the  
 conduct of nice operations; but the re-  
 sults, though attended with a little varia-  
 tion, which was evidently owing to the  
 trifling difference of certain circumstances  
 in the manner of performing the experi-  
 ment, confirmed the great discovery, viz.  
 that by the action of electric sparks, a mix-  
 ture of dephlogisticated and phlogisticated  
 air is converted into nitrous acid, capable  
 of

of forming nitre when combined with an alkali.

In the first volume of this treatise, the principal mechanical effects produced upon water by the electric sparks have been sufficiently noticed ; but some remarkable experiments were made not long ago, in which a decomposition of water was effected by the mere action of electric shocks, which converted it into an inflammable and permanently elastic fluid, and this elastic fluid being afterwards inflamed by the same electric spark, was thereby again converted into water. In order to give my readers a just account of those valuable experiments, I shall subjoin the translation of a letter of the authors themselves, in which their experiments and observations are methodically and clearly described.

*LETTER of Messrs. PACTS, VAN TROOSTWYK and DEIMAN, to Mr. DE LA METHERIE, on a new Method of converting Water into dephlogisticated and inflammable Air.*

“ S I R,

“ We request of you to insert in your journal, the experiments and observations which we have the honour of transmitting to you, upon a subject the most famous in chemistry and natural philosophy.

“ Howsoever satisfactory those experiments may appear, upon which Mr. LAVOISIER, and most of the French chymists have established their theory concerning water, it must nevertheless be acknowledged that they are not absolutely conclusive,

“ The partizans of each of the two opposite theories at present agree in the following particulars: 1st. That by the combustion of inflammable and dephlogisticated air some acid as well as water is obtained:

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2dly,



2dly, That this acid was not accidentally contained in the airs employed, but that it is really formed in the act of inflammation. The same thing however cannot be said with respect to the water; for this may be supposed to have previously existed in the two airs, which may be principally proved by observing, that the dryer the airs are made before the combustion, the smaller is the quantity of water obtained after it. Upon the whole, it seems, that the adversaries of the new theory have as much right to call the water an accidental substance, as its defenders have for considering in that light the acid which is found after the combustion.

“ The decomposition of water, provided it could be clearly proved, might decide this question without leaving room for the least difficulty. Water has hitherto been decomposed only by the aid of iron, a substance which, by the action of heat alone, will produce a sort of air, which is looked upon as an elementary component of water. It may therefore be supposed, that in this experiment the water serves only to disen-

gage

gage the air more easily, and in greater quantity from the metal, which is naturally disposed to yield it. Even this theory of the decomposition of water is entirely founded upon an hypothesis, which is not generally admitted, viz. that the calcination of metals consists in nothing more than their combination with the base of dephlogisticated air. The calcination itself in this experiment seems not to have been satisfactorily proved, and many philosophers entertain doubts about it.

“ Notwithstanding what we have said, to shew that the new theory of the French chymists concerning water, has not been hitherto rigorously proved, and though we ourselves entertained the contrary opinion; we are, however, far from meaning to defend the old system. On the contrary, we believe that we may contribute considerably towards the establishment of the new theory; since we have been fortunate in the discovery of a method of decomposing water, and of converting it into inflammable and dephlogisticated air, in a manner which seems to exclude the possibility of those

those airs being derived from any other substance.

“ Being employed (together with Mr. Cuthbertson, who has assisted us considerably in these experiments, and whom we are very willing should share with us the merit of the present discovery) in an investigation of the effects of the electric shocks on different substances, we had the curiosity of observing its effects on water also. For this purpose we filled a tube of  $\frac{1}{8}$  of an inch in diameter and one foot in length, with distilled water. One extremity of the tube was hermetically sealed, and a gold wire was closed in it, which projected an inch and a half within the tube. The other extremity of the tube was immersed in a small glass vessel full of distilled water, and another wire passed through this aperture, and went up into the tube so as to be  $\frac{5}{8}$  of an inch distant from the first-mentioned wire. In order to transmit the electric shock from one of those wires to the other, and of course to let it pass through the water contained in the tube between the extremities of the two wires, we placed the  
closed

closed end of the tube against a copper-ball, that stood insulated at some distance of the prime conductor of our machine, and a communication was made from the extremity of the wire which stood in the vessel full of water, to the outside of a Leyden phial, the coated surface of which was equal to one square foot, and whose knob communicated with the prime conductor\*.

“ Trying with this apparatus the effects of the electric shock upon the water, the copper-ball standing at a very small distance from the prime conductor, we could not perceive any production of air. Having increased this distance, and consequently the strength of the shock, we could see at each explosion a spark on the extremity of each wire, and at the same time a great number of very small bubbles of air appear-

\* The electrical machine which we have used for those experiments, consists of two plates, the diameter of each of which is 31 inches, and its construction is like that of TEYLER. (See p. 273. vol. II. of this treatise.) Its power is such as that a Leyden phial, like the one mentioned above, will be charged, and spontaneously discharged 25 times in 15 revolutions of the plates of the machine.

ed like a continue stream between those two extremities. The production of air was more abundant and the bubbles were larger, when the copper-ball was placed at a distance still greater from the prime conductor, in which case a ray of light was seen to proceed into the water from the extremity of the upper wire. The air which was thus produced, ascended to the upper part of the tube, where it formed a column, which kept increasing in proportion as we continued to transmit the electric shocks through the water, till it reached the extremity of the upper wire, at which time the electric spark, which was now obliged to pass through it in its way to the other wire, inflamed it, precisely in the same manner as it does with inflammable air, and caused it to disappear, excepting a very small residuum. Having removed that residuum, we again transmitted the electric shocks through the water, which produced another quantity of air, and when the column of it had reached the extremity of the upper wire, the inflammation took place, and the air disappeared all but a small residuum, every thing happening exactly as in  
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the preceding experiment. We repeated the same experiment many times over, and constantly observed the same phenomena, excepting that the residuum of air remaining after the inflammation in each experiment, seemed to be smaller than in the preceding.

“ We cannot therefore be any longer in doubt of having obtained inflammable air from water, viz. the same species of air, the base of which has been looked upon as a constituent element of water. The explosive property of this air, which, as it is well known, cannot take place without the presence of dephlogisticated air, seems to point out the probability of the latter sort of air being likewise produced at the same time. It is however necessary, in order to remove every possible doubt, to prove in the first place, that the electric matter does not in the least contribute to the production of the inflammable air; and, secondly, that the respirable air, which enables it to take fire, is not to be attributed to any common air contained in the water or adhering to the surface of the tube. Here follow  
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the experiments which we have made with a view to elucidate those two points.

“ The method which we thought to be the easiest as well as the best calculated to clear the first difficulty, was to pass the electric shocks through oil of vitriol, and through the nitrous acid, in the same manner as we had passed them through water. When those acids were used instead of water, and especially with the vitriolic, the production of air did also take place, though not so abundantly as from the water: but when the quantity of this air was increased so far as to go beyond the extremity of the upper wire, the inflammation did not take place; instead of which, the augmentation proceeded in the same proportion. We therefore suspected that this might be the vitriolic acid air, or the nitrous acid air; and as it is known that those airs are absorbed by their respective acids, we left them in that situation for a considerable time; but their bulk did not suffer the least diminution. This air therefore could not be supposed to be any thing else besides the dephlogisticated air. In order to ascertain this point, we

we introduced a certain quantity of nitrous air into the tube, and observed, that as soon as this air came to be mixed with the air contained in the tube, a diminution of bulk ensued, like that which takes place whenever dephlogisticated air is mixed with nitrous air.

“ After having compared those experiments together, we conceive we have demonstrated, that the electric shock produced no other effect upon the water, besides disposing the base of inflammable air to assume its aeriform state; and that in the acids it causes the base of dephlogisticated air to assume that state also. If the electric shock itself had contributed something towards the formation of the inflammable air, it would not have detached from the acids the pure respirable air; since by the admixture of the inflammable principle, this air would have been contaminated: and on the contrary it would, agreeably to that principle, have produced the vitriolic acid air or the nitrous acid air. There remains, therefore, no doubt, but that the inflammable air, obtained from water, is derived from the water

ter itself, and that it is one of its constituent principles.

“ The next thing to be ascertained is, whether the dephlogisticated air, which is shewn to exist in it by the explosion of the inflammable air, derives its origin from the water itself, or from a portion of atmospheric air, that may either have been contained in the water, or may have adhered to the surface of the tube. For this purpose we judged it necessary, previous to making the experiment, to deprive both the water and the tube of whatever air they might contain, by means of the air-pump, invented by Mr. Cuthbertson, which machine is capable of producing a greater degree of rarefaction, than any other of the kind hitherto made public.

“ We then proposed to repeat the experiment many times over, and to remove, after every inflammation, the remaining small quantity of air; so that by repeating the production of air, and its inflammation, we might perhaps succeed to deprive the water of all the air, which, notwithstanding the

purification by the air-pump, might still be contained in it; and of course to let the entire quantity of air disappear after the inflammation, or else that if some atmospheric air was concerned in it, we might at last prevent the inflammation entirely.

“ Accordingly we repeated the experiments, and in order to prevent the possibility of the water absorbing any air from the atmosphere, we immersed the aperture of the tube in quicksilver. And to prevent the gold wire being touched by the quicksilver, when the tube was inverted for the purpose of letting out the residuum of air, we made use of another tube having the same diameter, and the same length as the first, but bent towards its lower part in the manner shewn by fig. 5th. The lower wire was of gold as far only as the inferior curvature, the rest being of platina. But this tube, and two others which we used after it, as well as the above-mentioned straight one, were all broken after a few explosions, which shewed, that the weight of the quicksilver made a great resistance to the expansion occasioned upon the water by the electric

electric shock, and therefore that it was scarcely possible to use quicksilver in such experiments. In consequence of which, we were under the necessity of immersing the open extremity of the tube again in water, but took care to use only water that had been deprived of air by means of the air-pump. We continued to use the bent tube, so that in case the atmosphere, by being in contact with the water of the small vessel, imparted to that water and to the water of the tube, a small portion of air, that air might not go farther than the upper part of the curvature of the tube. We derived another advantage from this method, which was, that by leaving a small quantity of air in this part of the tube, from which no error could be derived, the water might expand itself with more freedom; in consequence of which, stronger shocks could be used without the danger of breaking the tube, and with more advantage, since the strong shocks were attended with a more copious production of air; this expedient was moreover absolutely necessary, considering that the success of the experiment, and the truth of the consequences deducible therefrom,

are more complete in proportion as the operation is performed in a shorter time\*.

“ Instead of leaving the distance of  $\frac{3}{4}$  of an inch between the prime conductor, and the copper-ball which received the electric spark, and which was in contact with the upper part of the tube full of water, we increased that distance to one inch. At the same time, in order to prevent the passage of the electric matter in the form of a continue ray from one wire to the other (which circumstance we had observed frequently to occasion the breaking of the tube) we placed the lower wire at the distance of one inch and a quarter from the upper one. With this disposition of the apparatus, the production of air was so rapid, that after 600 shocks, the length of the column of air was one inch and  $\frac{2}{3}$ th, and had very nearly reached

\* The truth of this remark was confirmed by experience, for when we tried the experiment on the following day, we were obliged to produce the air twice, and to inflame it twice, before we could obtain a residuum so small as we had obtained on the preceding day, after a single inflammation. This difference must be undoubtedly attributed to the common air, which, during the interval of time, had insinuated itself into the water.

the



the extremity of the upper wire. But as the expansion of the water was now very considerable on account of the strong shocks, the column of air was thereby frequently interrupted; and lest this accident should happen when the column of air had reached the extremity of the wire, which would have occasioned a partial inflammation of the air, we thought proper to diminish the force of the electric shocks. In this manner we proceeded, till the air had reached the extremity of the wire, at which time the inflammation took place, and the column of air disappeared, excepting a small bubble of  $\frac{1}{16}$ th of an inch in diameter. The tube was then inclined, so as to let the said bubble come to the extremity of the wire, and by sending an electric shock through it, the inflammation again took place, and half of the bubble disappeared.

“ Having expelled this small residuum, we began to produce the air anew, which was obtained with the same rapidity. The operation was continued till the air reached the extremity of the wire. The residuum, after the inflammation, was not more than

$\frac{3}{8}$ th of an inch in diameter. A second inflammation, effected in the above-mentioned manner, reduced the half of this residuum to

" The third time the residuum was not more than  $\frac{1}{8}$ th of an inch in diameter, but it proved difficult to inflame it a second time. We however succeeded at last, and there remained a bubble extremely small, the diameter of which seemed to be about  $\frac{1}{16}$ th of an inch.

" The fourth time (the air having always been produced with the same rapidity) all the column disappeared, excepting a small bubble of about  $\frac{1}{8}$ th of an inch in diameter, which it was found impossible to inflame a second time ; but if we consider the very small size of this remaining bubble when compared to the total quantity, and likewise if we admit that the half of this bubble might, in all probability, have been diminished by a second inflammation, provided it had been practicable, we cannot but think ourselves authorized to suppose, that the air produced is not by any means to be attributed

buted to any common air contained in the water, or at least only to an exceeding small quantity; which, in the first case, would have intirely disappeared, and this must have also happened if we could have proceeded farther with the successive inflammations.

“ We beg leave to add some remarks on these experiments, and upon others that have formed the support of the new theory concerning water, which theory has been confirmed by our experiments.

“ We believe we have completely demonstrated by our experiments, that water consists of the bases of inflammable, and of dephlogistified air. But let us collect together the principal points.

1. “ We have obtained inflammable air, which is the only sort of air capable of combustion; and dephlogistified air, which is the only species of air that can assist the combustion. This has been proved by the inflammation.

2. " We have obtained nothing but those two sorts of air, which is proved by the disappearance of the whole quantity of air after the last inflammation.

3. " The inflammable air has been really produced from the water; for if the electric fluid had in any other manner contributed to the formation of the airs thus obtained, it would not have furnished, under the same circumstances, two species of air so dissimilar as the dephlogisticated and the inflammable airs. This last mentioned air therefore is to be entirely attributed to the water.

4. " The dephlogisticated air must likewise derive its origin from the decomposition of the water, for if it were owing to some air accidentally contained in the water, it would not be purer than common air, which contains only a certain portion of dephlogisticated air. In that case, the residuum could not have been diminished by the successive inflammations, nor would it have entirely disappeared.

5. " Lastly,

5. “ Lastly, As the air obtained from the water by means of electricity, is a mixture of inflammable and dephlogistated air, and as the inflammation converts this air again into water, the same experiments afford a synthetick, as well as an analytick, demonstration, of the theory advanced concerning water. It naturally follows from those experiments, that the portion of acid, which has always been found together with water after the usual combustion of inflammable and dephlogistated air, from which the opponents of the new theory have derived a strong argument in their favour, must be considered as an accidental production, which cannot possibly invalidate the propriety of the consequences deduced from the experiments.

“ The adherents to the new theory have long since explained, in a very satisfactory manner, the formation of this acid, which is known to be the nitrous, in the combustion of the two airs; by attributing it to a combination of dephlogistated air with some phlogistated air, which may always be supposed to exist in dephlogistated air, as  
this

this sort of air cannot be obtained perfectly pure by any known method. As some difficulties have been started against this theory, we think it not improper to add a few short observations in reply, though they may not be immediately connected with the principal subject of this letter.

“ It has been objected, 1st, That the purer the residuum is after the combustion, (which shews that the dephlogisticated air employed must have been purer and freer from phlogisticated air) the stronger is the acidity of the liquor obtained ; and on the contrary, that when the residuum contains more phlogisticated air, the liquor which is obtained approaches nearer to the nature of pure water. 2d, That by adding phlogisticated air to the mixture of the two airs, the acidity of the liquor is diminished ; so that if instead of dephlogisticated air, common air be used, which contains a considerable portion of phlogisticated air, the acidity will hardly be perceivable.

“ In order to explain the first difficulty we must observe, that the dephlogisticated  
air



air has a greater affinity to inflammable than to phlogisticated air, and that it never combines with the latter, unless it cannot meet with a quantity of inflammable air sufficient for its saturation. Let us therefore consider the differences which must arise, from using various proportions of inflammable and dephlogisticated air.

“ Let the quantity of inflammable air be invariably the same, and let the dephlogisticated air vary in quantity ; we must likewise suppose that the latter does always contain a portion of phlogisticated air, which, by combining with the dephlogisticated air, may form the nitrous acid, from whence the liquor obtained derives its acidity.

1. “ If the quantity of dephlogisticated air be not greater than what may be fully combined with the inflammable air, the liquor will consist of nothing but pure water, and the residuum will be phlogisticated air.

2. “ If there be an excess of dephlogisticated air, the superfluous quantity of it will

will combine with the phlogisticated air, and with it will form the nitrous acid. The liquor thereby obtained will consist of water and acid; and the remainder, if there be any, will consist of nearly pure dephlogisticated air.—Both those particulars are confirmed by Mr. Cavendish's experiments.

3. "The second objection is of an equivocal nature; for if by the addition of phlogisticated air, it be meant, that the mixture of the two airs is increased by the addition of phlogisticated air, it is evident that this addition cannot occasion any alteration in the result, because when the dephlogisticated air meets with a great quantity of inflammable air, it will not combine with its own portion of phlogisticated air, and of course it will much less combine with the phlogisticated air that has been added. This is the reason why Dr. PRIESTLEY's experiments, when he added phlogisticated air to the mixture of the two airs, were not attended with any peculiar effect. The same reasoning must be adopted when atmospheric air is used instead of dephlogisticated air; for when the quantities are alike,

alike,

alike, either no particular effect will take place, or the acidity of the liquor will be very slight, and the residuum will be more impure. If, on the contrary, the phlogisticated air be added, and at the same time the quantity of inflammable air be diminished, whilst that of the dephlogisticated air remains the same; a portion of the latter, becoming superfluous, will combine with the phlogisticated air, and with it will form the acid. The residuum itself may, in great measure, consist of dephlogisticated air. Such has been the result of Mr. CAVENDISH's experiments, who has proceeded in the above-mentioned manner. It appears that for this reason such results have been totally different from those of Dr. PRIESTLEY.

“ We must confess that there remains to be explained, why the combination of dephlogisticated and phlogisticated air takes place in the said combustion; whilst the electric shock is the only means by which that combination can be effected. We have felt the force of this difficulty too late, and when it was no longer in our power to clear

it by a particular set of experiments; but we are led to believe, that the cause of it must be attributed to the great quantity of light which is manifested at the time of the inflammation. Mr. BERTHOLET has discovered, that the nitrous acid, when exposed to the light, yields dephlogisticated air. We have observed this effect of light, not only upon the nitrous, but likewise on the vitriolic acid, when exposed to the focus of a burning-glass; and we have observed the same effect, when the electric shock has been applied to those acids. Having therefore remarked this analogy between light and electricity in the decomposition of acids, it seems probable, that the light manifested by the inflammation may produce an effect analogous to that of electricity, in the formation of nitrous acid, viz. to produce that acid by the combination of dephlogisticated and phlogisticated air.

“ After a review of those observations, which convert one of the strongest objections to the new theory concerning the nature of water, into an argument in favour of the same theory; there seems to remain no  
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doubt

doubt with respect to the nature of water; and we may conclude with saying, that it consists of the bases of dephlogisticated and phlogisticated air.

“ We are, &c.”

It is necessary to add, for the satisfaction of my readers, that most of the valuable experiments which are mentioned in the preceding letter, having been repeated in London, have been found to answer in the manner above stated with all the accuracy which can be expected in such cases. Some of the observations might perhaps require farther elucidation and investigation; but this examination would lead us too far into the doctrine of permanently elastic fluids, which is foreign to the subject of this work.

OF THE  
 R E P U L S I O N  
 BETWEEN BODIES POSSESSED OF THE SAME SORT OF  
 E L E C T R I C I T Y ;

AND

Of some EXPERIMENTS, which seem to militate against  
 the Theory of a single Electric Fluid.

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**T**HE principal phenomenon, which, in the opinion of many writers, cannot be explained by the Franklinian theory of a single electric fluid, is the repulsion which takes place between bodies negatively electrified ; and, notwithstanding what has been said about it in the preceding editions of my Treatise, I find that in some recent publications the said repulsion is still supposed to contradict that theory. It seems therefore necessary to examine and to explain that



that phenomenon in the following more particular manner :

*Proposition I.—No electricity can appear on the surface of a body, or no body can be electrified either positively or negatively, unless the contrary electricity can take place on other bodies contiguous to it.*

This proposition may be proved by a great many experiments and observations, such as may be seen in Chap. V. and VI. Part I.; and in pages 199, 201, 212, 248, 331, of Vol. I.; also in page 247, &c. of Vol. II. of this work.

*Proposition II.—There is something on the surface of bodies which prevents the sudden incorporation of the two electricities, viz. of that possessed by the electrified body, with the contrary electricity possessed by the contiguous air, or other surrounding bodies.*

Without examining the nature, the extent, and the laws of this property in bodies, it will be sufficient for the present purpose to observe, that the fact is certainly

so ; for otherwise a body could not possibly be electrified, or it would not remain electrified for a single moment.

Proposition III. — *Supposing that every particle of a fluid has an attraction towards every particle of a solid; if the solid be left at liberty in a certain quantity of that fluid, it will be attracted towards the common centre of attraction of all the particles of the fluid.*

Let the body be extremely small, and it is evident that it must be drawn towards the common centre of attraction ; for if it be placed on one side of the said centre, the attractive particles on the opposite side, being more numerous, will naturally draw it that way. If the body be large, the same reasoning shews that the effect must be the same ; for the attractive force of all its particles being concentrated in a point or centre, will draw that centre towards the centre of attraction of all the particles of the fluid. Thus, if the fluid be of a spherical form, and the solid body be likewise spherical,

rical, the centre of the latter will coincide with that of the former.

Corollary 1.—The same thing must happen, when the quantity of fluid is smaller than the bulk of the body ; in which case the former must be within the latter.

Corollary 2. — If the attraction of the particles of the fluid be exerted only towards the surface of the solid, and not towards its internal parts, the effect will be the same ; when the body is of a regular shape, as spherical, cubical, &c. but with very irregular shapes, the difference will be very trifling, and not deserving of notice in this place.

The application of the foregoing propositions, in explanation of the repulsion which takes place between bodies possessed of the same sort of electricity, is very easy, and, in my opinion, conclusive. According to the Franklinian hypothesis, the electric fluid is elastic or repulsive of its own particles, and attractive of the particles of other matter. Let then A and B, fig. 7, be two spheres

of metal suspended in the open air contiguous to each other, and capable of being easily moved. Let some electricity be communicated to them, and by Proposition I. it will appear, that whilst the bodies touch each other, as shewn in fig. 7, the electricity which is communicated cannot be dispersed equably all over their surfaces, but it must be thicker or more condensed on the parts that are remote from the mutual point of contact, because there the air is at liberty to acquire a contrary electricity; whereas near the point of contact the electricity cannot be manifested, because in that place there is no air or other body that can acquire the contrary electricity. Therefore the atmospheres of contrary electricity cannot be concentric with the spheres A and B; but must be situated as in fig. 7. It follows therefore by Proposition III. that the spherical bodies being attracted towards the centres of those atmospheres, appear to repel each other, as shewn in fig. 8; so that when the bodies are electrified positively, in which case, according to the hypothesis, they have acquired an additional quantity of electric  
8 fluid,

fluid, negative atmospheres will be formed round them, and the additional or superfluous electric fluid of the bodies will attract, and be attracted by, those negative atmospheres. And when the bodies are electrified negatively, in which case, according to the hypothesis, they have lost part of their usual quantity of electric fluid, positive atmospheres will be formed round them, which will attract the under-charged bodies.

What has been exemplified in the two metallic bodies, may be easily applied to explain the electrical repulsion between any number of bodies, and to bodies of any shape and substance ; proper allowance being made for their conducting or non-conducting nature, and for their weight, which may render them more or less susceptible of the action of electricity.

The other experiments, which have been considered as repugnant to the Franklinian hypothesis, may be almost all reduced to this, viz. that in making the discharge of a Leyden phial, not highly charged, through a

long circuit, the effect of the discharge is more sensibly felt by those parts of the circuit which lie near the two coatings of the phial, than by the middlemost parts of the circuit which lie farther off. But very little consideration is required, to shew that the explanation of that phenomenon, upon the above-mentioned hypothesis, is not attended with any difficulty. I shall, however, for the sake of perspicuity, describe two or three of those experiments, and shall afterwards subjoin a general explanation.

Experiment 1.—Charge a Leyden phial very weakly, viz. so as just to afford a visible spark when discharged; then apply a finger of one hand to the outside, and a finger of the other hand to the inside coating of it. The effect will be, that the slightest pricklings will be felt on these fingers, and no where else. Charge the phial a little higher, then apply the fingers to its two coatings as before, and a sharper sensation will be felt all along the two fingers. If the phial be charged still higher, the sensation will be felt as high as the wrists;



wrists; with a greater charge, the sensation will be felt in the arms, &c.

Experiment 2.—Insulate a great number of metallic balls or bars, or, in short, many conducting substances, and dispose them so as to be within a small distance of each other, but not in actual contact. The distances between any two contiguous bodies ought to be equal, which may be easily done by interposing a card, or something else of a proper thickness, when the bodies are situated in their places, but not to remain between them. Let this interrupted circuit form the communication between the two sides of a charged Leyden phial; and it will be found, that when the charge of the phial is very weak, sparks will be seen between those interruptions of the circuit which are near the two coated surfaces of the phial; if the charge be higher, the sparks will be extended through more interruptions; and if the charge be still higher, the sparks will be seen through all the interruptions of the circuit.

Experiment 3.—Take a pretty long glass tube furnished with metallic caps at both ends, one of which caps must have a stop-cock, and exhaust it of air by means of an air-pump. Then let this exhausted tube form part of the circuit between the inside and outside of a charged Leyden phial; and it will be found, that when the charge of the jar is very weak, the two ends only of the tube will be illuminated; whereas when the charge is sufficiently high, the light will pervade the whole cavity of the tube from end to end.

In these and many similar experiments, those persons who wish to find fault with the hypothesis of a single electric fluid, imagine that a double current, and two distinct powers, must necessarily exist. They are satisfied with the slight evidence of their senses, and do not give themselves the trouble of considering the matter any farther.

In order to shew, that those appearances are perfectly and unequivocally explainable on the theory of a single electric fluid, I shall

shall just mention two known truths, viz. 1st, That the condensation of an elastic fluid, such as the electric fluid is supposed to be, is inversely as the spaces in which it is confined. Thus, when a certain quantity of it is confined in half the space, it is then said that its condensation is double of what it was before; when it is confined into the tenth part of the original space, the condensation is ten times greater, and so on. 2dly, That the effects produced by a certain quantity of electricity, such as the spark, shock, &c. are proportional to its condensation. Thus, the highest charge of a pint phial will give a man a much greater shock than he would wish to receive; yet if that charge be communicated to a battery of one hundred square feet, and the same man apply his hands to the two sides of that battery, he will hardly feel the shock, because the same quantity, which in the former case was confined into a small space, loses the greatest part of its power when it comes to be rarefied into a much larger space.

Thus much having been premised, let us consider the second experiment, and let

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us suppose that the inside and outside coated surfaces of the Leyden phial are each equal to five square inches ; that before the charge, each of these surfaces contained five parts of electric fluid, viz. one for every square inch ; but after the charge, the inner surface acquired four additional parts of it, so as to make all together nine, and the outside lost four of its original number, in consequence of which one part only of electric fluid remained in it. We must likewise suppose, that each of the bodies which form the interrupted circuit, has a surface of one square inch, and contains one part of electric fluid. Now, when the two coatings of this charged phial come in contact with the extremities of the interrupted circuit, the inside endeavours to discharge its four parts of superfluous electric fluid, and the outside endeavours to recover its four parts of lost electric fluid. By the contact of the body next to it, the inside of the phial, together with that body, will form a surface of six square inches, and the whole quantity of electric fluid contained in it, is ten parts ; but ten parts of fluid will have more room in six square inches, than nine parts

parts had in five square inches ; therefore the electric fluid is less condensed now than it was before the contact of the first body. Further, when the electric fluid has reached the second body of the circuit, the whole quantity of fluid will be eleven parts, and the whole quantity of surface will be seven square inches ; but eleven parts of fluid in seven inches of surface, will have more room than ten parts in six inches of surface ; therefore by the contact of the second body of the circuit, the electric fluid becomes still more rarefied, and so on ; the condensation therefore, and, of course, the intensity of the spark and other effects, diminish in proportion as the electric fluid proceeds in the circuit farther and farther from the coated surface of the Leyden phial.

If the same reasoning be applied to the outside negative surface of the phial, it will in the like manner appear, that the attraction of that surface towards the electric fluid, is diminished in proportion as the bodies of the circuit successively add their natural share of fluid to the joint surface, viz.  
the

the surface of the phial, together with their own surface. Therefore when the Leyden phial is weakly charged, the sparks will appear only between those bodies which are contiguous to its surfaces, or, in general, the effects of the discharge are more remarkable near the phial, and are less conspicuous at a greater distance from it.

The peculiar merits of different hypotheses on any philosophical subject whatsoever, are not to be derived from those phenomena which admit of an easy explanation upon different suppositions, but they must be determined by those which can only be explained upon one theory, and no other. Therefore the foregoing experiments will not render the hypothesis of a single electric fluid more probable than some other electric hypothesis; but the experiments which render that hypothesis more likely to be true, are those which shew an evident current from the positive to the negative side in the discharge of a Leyden phial, or, in general, a current from a body positively electrified, and towards a body negatively electrified;



electrified\* ; since those experiments do not admit of an easy explanation on other hypotheses. Therefore the object of this section has been only to shew, that those experiments which are considered as contradictory to the theory of a single electric fluid, are so far from contradicting it, that they may be clearly and satisfactorily explained by it.

\* The reader will find many experiments of this sort in the first and second volumes of this Treatise.

## R E M A R K S

ON SOME EXTRAORDINARY EFFECTS OF

T H U N D E R S T O R M S ;

AND AN EXPLANATION OF THE

ELECTRICAL RETURNING STROKE.

ONE of the most instructive accounts of thunder storms that have appeared in print, was drawn up some years ago by PATRICK BRYDONE, Esq; F. R. S. and was published in the 77th volume of the Phil. Trans. The remarkable effects contained in that account, and the accurate manner in which they were observed and described, renders it so highly interesting to electricians, that I shall transcribe the principal part of it in this place, and shall then subjoin the best conjecture which has been hitherto offered in explanation of the phenomena therein contained.

“ Tuesday

“ Tuesday the 19th of July, 1785, was a fine soft morning (thermometer at ten, 68°.); about eleven, clouds began to form in the south-east; and between twelve and one there were several flashes of lightning, followed by rolling claps of thunder at a considerable distance. I was sitting in my study at an open window, in the second story, observing the progress of the storm; when some ladies, who were in the drawing-room below, alarmed by the lightning, came up to me. I was making them observe, by a stop-watch, the time which the sound took to reach us (which was generally from 25 to 30 seconds) and assuring them the storm was at so great a distance, that there could be no sort of danger; when we were suddenly alarmed by a loud report, for which we were not prepared by any preceding flash: it resembled the firing of several muskets, so close together, that the ear could hardly separate the sounds, and was followed by no rumbling noise like the other claps.

“ The clouds immediately began to dissipate, and there was no more appearance  
2 of

of either thunder or lightning. I had ordered my horses to be got ready, and was just going to mount, when a servant came running in to tell me, that a man and two horses had been struck dead by the thunder, at a small distance from the house. I immediately set out, and arrived on the spot in less than half an hour after the accident. The horses were still yoked to the cart, and lying in the same position in which they had been struck down; but the body of the young man had been already carried off by his companion, who soon returned to the place; and, with less agitation than I expected, described to me how every thing had passed.

“ They were both servants to Mr. Turnbull, a tenant of the Earl of Home, and were returning home with two carts loaded with coals. James Lauder, a strong young man, of 24 years of age, had the charge of the first cart, and was sitting on the fore-part of it. They had crossed the Tweed a few minutes before, at a deep ford, and had almost gained the highest part of an ascent, about 65 or 70 feet  
above

above the bed of the river. They were conversing about the thunder, which they heard at a distance, and expressing a wish that it might be accompanied by a fall of rain, as the only means of saving the crop, after so long and so severe a drought. At that instant he was stunned by a loud report, and saw his companion, his horses and cart, fall to the ground. He immediately ran to his assistance, but found him quite dead. His face, he said, was of a livid colour, his clothes were torn to pieces, and he had a strong smell of burning. He immediately emptied his own cart, and carried home Lauder's body to his friends; so that I had not an opportunity of examining it: but Mr. Bell, Minister of Coldstream, a gentleman of the most perfect candour and veracity, told me, that he had been sent for, to announce the fatal event to the young man's parents, and had examined the body: that he found the skin of the right thigh much burnt and shrivelled, and many marks of the same kind over the whole body, but none on the legs, which he imputed to their hanging over the fore-part of the cart at the time of the explosion, and not being in contact

with any part of it. His clothes, and particularly his shirt, was very much torn, and emitted a strong smell of burning. The body was buried two days after, without having discovered any symptoms of putrefaction.

“Lauder’s companion shewed me the distance between the two carts, which was exactly marked; for his horses had turned round at the time of the explosion, and broke their harness: I found it about 24 yards, and Lauder’s cart was a few feet higher on the bank, but had not yet reached the summit. He told me, he was likewise sitting on the fore-part of his cart, and had Lauder, his cart and horses, full in view, when they fell to the ground; that he perceived no flash nor appearance of fire, and was sensible of no shock, nor uncommon sensation.

“I now examined the cart, and the spot round it, as exactly as I could. The horses were black, and of a strong make; they had fallen on the left side, and their legs had made a deep impression in the dust, which,  
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On our lifting them up, shewed the exact form of each leg; so that no kind of struggle or convulsive motion had succeeded the fall, but every principle of life seems to have been extinguished in an instant. The hair was much singed over the greatest part of their bodies, but was most perceptible on the belly and legs. Their eyes were already become dull and opake, and looked like the eyes of an animal which had been long dead. The joints were all supple; and I could not perceive that any of the bones were either softened or dissolved, as it has been alledged sometimes happens to animals killed by lightning. The left shaft of the cart was broken; and I observed, that splinters had been thrown off in many places, particularly where the timber of the cart was connected by nails, or cramps of iron. Many pieces of the coal were likewise thrown out to a considerable distance, all round the cart; and some of them which I have preserved, have the appearance of coal which had lain some time on a fire. I likewise gathered up the fragments of Lau-

shall inclose for your inspection, as well as part of his hair, which I found strongly united to some of the fragments which had composed the crown of the hat. About four feet and a half behind each wheel of the cart, I observed an odd appearance in the ground; a circular hole of about twenty inches in diameter, the centre of which was exactly in the track of the wheel. The earth was torn up as if by violent blows of a pick-axe, and the small stones and dust were scattered on each side of the road. The tracks of the wheels were strongly marked in the dust, both behind and before these holes, but were completely obliterated for upwards of a foot and a half on these spots. This led me to suspect, that the force which had formed them, must likewise have acted strongly upon the wheels; and, on examination, I found evident marks of fusion on each of them, which I now shewed to many people who had assembled around us. The surface of the iron, to the length of about three inches, and the whole breadth of the wheel, had become of a bluish colour, had entirely lost its polish and smoothness, and had the appearance of  
drops

drops incompletely formed on its surface; these were of a roundish form, and had a sensible projection. I suspected that the great heat which had been communicated to the iron, might probably have burnt the wood of the wheels; but this I did not find to be the case. To ascertain whether these marks were occasioned by the explosion which had torn up the ground, we pushed back the cart on the same tracks which it had described on the road; and found, that the marks of fusion answered exactly to the centre of each of the holes; and that, at the instant of the explosion, the iron of the wheels had been sunk deep in the dust. They had made almost half a revolution after the explosion, which might be occasioned by the falling down of the horses, which pulled the cart a little forward. On examining the opposite part of the wheels, or that part which was at the greatest distance from the earth, no mark of any kind was observable. The broken earth still emitted a smell something like that of ether. The ground was remarkably dry, and of a gravelly soil.

“ It would appear, that this great explosion had, in an instant, pervaded every substance connected with the cart, the wheels of which had probably conducted it from the ground. They had been completely wetted but a few minutes before, as well as the legs and bellies of the horses, and this might, perhaps, be the reason why the hair on these parts was much more burnt than on the rest of their bodies. However, the two horses had already walked over this electrical mine, without having produced any effect; and had not the cart followed them might have escaped without hurt. I examined all their shoes, but could not perceive the least mark on any of them, nor was the earth broken where they had trodden; but the cart was deeply laden, and the wheels had penetrated much farther into the ground.

“ The equilibrium between the earth and the atmosphere seems at this instant to have been completely restored; for no farther appearance of thunder or lightning was observed within our hemisphere; the clouds dispelled, and the air resumed the  
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most perfect tranquillity: but how this vast quantity of electric matter could be discharged from the one element into the other without exhibiting any appearance of fire, I shall not pretend to examine. The fact, however, appears certain, and when I was mentioning it as a singular one, a gentleman told me, that the shepherd of St. Cuthber's farm, on the opposite bank of the Tweed, had been an eye-witness of the event, and gave a different account of it. I immediately went to the farm, found the shepherd, and made him conduct me to the spot from whence he has observed it, and desired him to give me an account of what had happened. He was looking, he said, at the two carts going up the bank, when he was stunned by a loud report, and at the same instant saw the first of the carts fall to the ground, and observed that the man and horses lay still, as if dead. I asked him, if he had observed any lightning? He said, he saw no lightning, nor appearance of fire whatever; but observed the dust to rise at the place; that there had been several flashes of lightning some time before from the south-east, whereas the accident

happened to the north-west of where he stood. The distance, in a right line across the river, might be between two and three hundred yards. He was sensible of no shock, nor uncommon sensation of any kind. I went next morning to examine if there were any marks of putrefaction on the horses, and to observe the state of the blood-vessels, &c. after the skin had been taken off; but a gentleman of the neighbourhood, who kept a pack of hounds, had already seized on them.

“ Several other phenomena happened on that day, probably all proceeding from the same cause; some of which I shall beg leave to mention.

“ The shepherd, belonging to the farm of Lennel Hill, was in a neighbouring field, tending his flock, when he observed a lamb drop down, and said, he felt at the same time as if fire had passed over his face (this was his own expression) although the lightning and claps of thunder were then at a great distance from him. He ran up immediately, but found the lamb quite dead; nor did he



he perceive the least convulsive motion, nor symptom of life remaining, although the moment before it appeared to be in perfect health. He bled it with his knife, and the blood flowed freely. This, he told me, happened about a quarter of an hour before the explosion which killed Lauder ; and it was not above 300 yards distant from the spot. He was only a few yards from the lamb when he fell down. The earth was not torn up, nor did he observe any dust rise.

“ Thomas Foster, a celebrated fisher in Coldstream, and another man, were standing in the middle of the Tweed, fishing for salmon with a rod, when they suddenly heard a loud noise ; and, turning round to see from whence it came, they found themselves caught in a violent whirlwind, which Foster told me felt sultry and hot, and almost prevented them from breathing. It was not without much difficulty they could reach the bank, where they sat down, exhausted with fatigue, and greatly alarmed : however, it lasted but a very short time, and was succeeded by a perfect calm. This  
happened

happened about an hour before the explosion.

“ A woman, making hay near the banks of the river, fell suddenly to the ground, and called out to her companions, that she had received a violent blow on the foot, and could not imagine from whence it came. This I had not from the woman herself, but from Mr. Turnbull, a very respectable farmer. Mr. Bell, our Minister, nephew of Thomson the poet, and possessed of all the candour and ingenuity of his uncle, told me, that, walking in his garden, a little before Lauder’s accident, he several times felt a sensible tremor in the ground. He likewise told me (what I find I had forgot to mention in the proper place) that he had observed on Lauder’s body a zig-zag line of about an inch and a quarter broad, which extended from his chin down to his right thigh, and had followed nearly the line of the buttons of his waistcoat. The skin was burnt white and hard.

“ These, Sir, are all the circumstances I have been able to collect that are well authenticated ;

thenticated; and I shall not trouble you with reports that are not. From the whole it would appear, that the earth had acquired a great superabundance of electrical matter, which was every where endeavouring to fly off into the atmosphere. Perhaps it might be accounted for from the excessive dryness of the ground; and, for many months, the almost total want of rain, which is probably the agent that Nature employs in preserving, or in restoring, the equilibrium between the other two elements. But I shall not pretend to investigate the causes: all I wanted, was to give you some account of the effects, and your own reflections will lead you much farther than any thing I could suggest."

After an attentive consideration of this account, several of the appearances observed in it, seem to furnish considerable instruction, and at the same time to require an attentive investigation of the causes from which they are likely to have originated. The most singular circumstance is, that notwithstanding the greatness of the effect, though a man and two horses were struck  
dead

dead by the matter of lightning, yet this great discharge of electrical matter was not attended with any appearance of light, no flash of lightning was observed by spectators, who, it seems, ought to have unavoidably perceived it, if there had been any. Whether the superabundance of electric fluid existed in the clouds or in the earth, if the discharge was made through the air, a very intense light would have certainly accompanied it ; and if the discharge was not made through the air, after it had gone through the man and horses, how could it be attended with so loud a report as the witnesses assert to have heard at the moment of the accident ?

The other phenomena, related at the end of the account, did also happen without any appearance of light ; at least it is not mentioned that there was any, and we may presume, that it would have been noticed if there had. However, in the affair of the lamb, the shepherd who saw it drop, said, that he felt at the same time as if fire had passed over his face ; but from this indefinite expression we cannot clearly understand,

stand, whether what he perceived was heat, light, or both heat and light together.

That the earth about that spot was charged with electrical matter, and that all the phenomena related in the account were effected by that cause, seems not to admit of a doubt; yet the superincumbent clouds had great share in the production of those phenomena; and they either produced the accumulation of electric matter in that part of the earth by the action of their electric atmospheres, or they themselves were drawn near, and became instrumental in the restoration of the electrical equilibrium.

The famous Italian philosopher, Fr. Beccaria, was of opinion, that very great quantities of electric matter were sometimes discharged from the earth in some particular spots, and, rushing into the atmosphere, drove in their way sand, ashes, leaves of trees, and other light matters; and that without the interference of any wind, or sometimes even against the direction of the wind, as he had more than once observed.

observed. But independent of this phenomenon, he was led to suppose, that the earth became over-charged with electricity in some particular places, and under-charged in others, and that the equilibrium between those differently electrified places was restored through the clouds; he being otherwise unable to account for the vast quantities of electricity, which were successively discharged, for a considerable length of time, from clouds of a limited extent.

Had the production of electricity from evaporation, and from condensation, been known by Fr. Beccaria; he would perhaps have not wondered at the successive production of lightning from clouds in a thunder-storm; for the clouds must not be considered as conductors simply electrified, which remain less and less electrified in proportion as sparks are drawn from them; but they acquire electricity from their very condensation or rarefaction, and this production of electricity, being proportionate to the quantity and quickness of the condensation or rarefaction, must be very great in certain cases, especially when the clouds are quickly  
converted



converted into rain, which is generally the case in thunder-storms.

Notwithstanding this observation, it is not unlikely but that Fr. Beccaria's hypothesis may sometimes be true, viz. that one part of the earth may become over-charged, whilst another part becomes under-charged, and that the clouds may form a sort of conducting rod between those two places\* ; yet even upon Fr. Beccaria's supposition, the above-mentioned facts do not seem to admit of an explanation ; the want of light being irreconcilable to that hypothesis.

A short time after the reading of Mr. Brydone's account of the thunder-storm in Scotland at the Royal Society, Earl Stanhope presented a paper to the same society, containing remarks on the said account †. In that paper, Lord Stanhope endeavours to account for the various phenomena observ-

\* Fr. Baccaria supposed, that earthquakes were produced by the accumulation of vast quantities of electricity deep in the bowels of the earth.

† Phil. Transf. vol. 77th.

ed during the thunder-storm, and attributes them to what he calls the *electrical returning stroke*. But before we enter into an examination of his lordship's explanation of the phenomena, I shall avail myself of this opportunity to give my readers an idea of the nature of this returning stroke.

When an insulated body A is brought within the action of another body B, which is electrified either positively or negatively, but not within the striking distance; the body A will have the equilibrium of its electric fluid disturbed, in such a manner as to acquire the electricity contrary to that possessed by B, on that side which is nearest to B; but on the opposite side, it will acquire the same sort of electricity; and in a certain part between those two extremities the body will remain in a neutral state, viz. not electrified \*. On removing the electrified

\* Suppose that the insulated body A is a cylinder, placed with its axis in the direction of the electrified body B. Call that end of the electrified body which is nearest to the insulated body, F; call that end of the insulated body which is towards the electrified body, G; and call the remotest end of the same insulated body, H. Then  
Lord

electrified body B, A will recover its equilibrium, and appear unelectrified as before.

When instead of one body, two bodies, A and D, are separately insulated at a small distance from each other in the same direction with the electrified body B, and are so disposed that A may be just out of the striking distance of B, the effect will be, that by the action of the electric atmosphere of B, the body A will acquire the contrary electricity, and the body D will acquire the same sort of electricity as that of B; but this effect cannot take place unless a spark or sparks take place between the two bodies A and D. And if the electrified body B be removed, or its electricity be discharged, another spark will appear between A and D, which will restore those two bodies to their original unelectrified state.

Lord Stanhope has demonstrated, that if the density of the electricity of electrical atmospheres be inversely as the squares of the distance, the *neutral*, or unelectrified, point in the insulated body is a fourth point of an harmonical division of the line F G H, the other three points F, G, and H being given.—Principles of Electricity, Part V.

This spark is called by Lord Stanhope the *electrical returning stroke* \*. It is hardly necessary to subjoin, that when the body B is electrified positively, the returning stroke consists of the electric fluid, which originally belonged to A, but by the action of the electrified body B, having been driven into D, is now returned to its original place in A. But when B is electrified negatively, then the electric fluid of D is first driven into A, and then returns to its original place in D.

What we have exemplified with the two bodies may take place amongst any number of bodies, some or the greatest number of which being or not being insulated, as it may be easily conceived without any farther explanation.

Thus much having been premised, we may now add Lord Stanhope's explanation of the phenomena that attended the storm. His lordship first shews, that Lauder's

\* See Part the 7th of Lord Mahon's Principles of Electricity.

death, &c. could not be occasioned by any *direct main stroke of explosion*; since there was no lightning; and that, for the same reason, it could not be occasioned by any *transmitted main stroke of explosion*; and, consequently, that it could neither be occasioned by a *lateral explosion*. But, “ I think,” says he, “ from the different circumstances of this case, that the effects produced proceeded from electricity; and that *no electrical fire did pass* immediately either from the clouds into the cart, &c. or from the cart, &c. into the clouds.

“ It is evident that the electrical fire did pass (from the earth to the cart, or from the cart to the earth) through that part of the iron of the wheels, which was in contact with the ground.

“ From the splinters that had been thrown off in many places, particularly where the timber of the cart was connected by nails or cramps of iron, and from the various other effects mentioned in Mr. Brydone’s paper, it is moreover evident, that there was a violent motion of the elec-

trical fluid in all, or (at least) in different parts of the cart, and of the bodies of the man and horses, although there was no lightning.

“ Wonderful as these combined facts may appear, and uncommon as they certainly are in this country, they are, nevertheless, easy to be explained by means of that particular species of electrical shock, which I have distinguished in my *Principles of Electricity* (published in 1779) by the appellation of the *electrical returning stroke*; and although at the time I wrote that treatise, I had it not in my power to produce any instance of persons or animals having been killed in the very peculiar manner since related in Mr. Brydone’s paper, I did however (from my experiments mentioned in that book) venture to assert with confidence, that if persons be strongly superinduced by the electrical atmosphere of a cloud, they may (under circumstances similar to those explained in that treatise) receive a very strong shock, be knocked down, or be even killed, at the instant that the cloud discharges, with an explosion, its electricity,



electricity, whether the lightning falls near the very place where those persons are, or at a very considerable distance from that place, or whether the cloud be positively or negatively electrified.

“ I have also explained, in that treatise, how a still more singular effect might be produced, namely, how an explosion, which happens in one place, may cause in a second place (at a very considerable distance from the first place) a sudden returning stroke, which may knock down, or even kill, persons and animals at that second place, at the same time that other persons, or other animals, situated in a third place, that is even immediately between the first place where the lightning falls, and the second place (just mentioned) when the shock of the returning stroke happens, shall receive no detriment whatever.”

Lord Stanhope after this proceeds to explain the phenomena, and to reconcile them to his theory of the returning stroke. “ Now,” *says he*, “ let us suppose a cloud, eight, ten, or twelve miles in length (be

the same more or less) to be extended over the surface of the earth.

“ And let another cloud be situated between the above-mentioned cloud and the earth.

“ Let the two clouds be supposed to be charged (for instance) with the same kind of electricity, and to be both positive.

“ Let us further suppose, that the lower cloud be near the earth, only a little beyond the striking distance; and let a man, cart, and horses, be situated under that part of the cloud which is the nearest to the earth.

“ Now, let us suppose this cart to be ascending an hill, and to be in the situation described by Mr. Brydone, namely, to have almost gained the highest part of the ascent, and to be followed by another cart lower down the hill.

“ Let us suppose also, that the two clouds be near each other, perpendicularly over the place where the first cart was situate.

“ And

“ And let the remote end of the upper cloud approach the earth, within the striking distance, and suddenly discharge its electricity into the earth.

“ Things being situated as above described, let us examine what consequences must follow.

“ First, when the upper cloud discharges its electricity into the earth, the lower cloud must immediately discharge its electricity into the upper cloud at the place which is directly over the cart.

“ This accounts for the loud report of thunder that was unaccompanied by lightning near the carts. The *report* must be loud, from its being near ; but no *lightning* could be perceived at the place near the carts, by reason of the thick thunder-cloud being situated immediately between the spectator at the second cart and the place between the two clouds where the lightning was.”

Lord Stanhope after this shews what effect would be produced by the atmo-

sphere of the second cloud upon a conducting body insulated under it. Then, *says he*, “but if this conducting body, on the surface of the earth, be not insulated, or be but imperfectly insulated, then the whole of such body (from its being immersed in the electrical atmosphere of the *positive* cloud) will become *negative*, because part of the electricity of this conducting body will, in this case, pass into the earth. And this conducting body will become the more *negative*, as it becomes the more deeply immersed into the dense part of the elastic electrical atmosphere of the approaching thunder-cloud.

“Now when the positive lower cloud (in the manner above stated) comes suddenly to discharge, with an explosion, its superabundant electricity into the upper cloud, then the electrical atmosphere of the lower cloud will cease to exist; consequently, the electrical fluid, which had been gradually expelled into the common stock, from the conducting body situated upon the surface of the earth, must (by the sudden removal of the superinduced elastic electrical

electrical pressure of the electrical atmosphere of the thunder cloud) suddenly return from the earth into the said conducting body, producing a violent commotion, similar to the pungent shock of a Leyden jar in its sensation and effects.

“ It was by such a returning stroke that Lauder, and the horses that he was driving, were killed, they having become strongly *negative* previous to the explosion.”

The objection which naturally occurs on first hearing the above-mentioned explanation is, that the quantity of electric fluid usually contained in the body of a man, in the horses, &c. does not seem to be sufficiently great to produce the described phenomena. To this Lord Stanhope replies in the following manner :

“ No person, the least versed in the principles of electricity, can hesitate to assent to the proposition, that the *electrical returning stroke* must exist, under circumstances similar to those explained above. But it may be objected to me, that although all  
the

the aforeſaid effects of a returning ſtroke might take place in a ſmall degree, yet thoſe effects could not have been ſufficiently powerful to have killed Lauder, the horſes, and the lamb, or to have melted the iron of the cart-wheels; eſpecially, conſidering the ſmall quantity of electrical fluid that is contained in the body of a man, of a lamb, or of a horſe, or that is contained in any body of the ſize of a common cart; that is to ſay, conſidering the ſmall quantity of electrical fluid that could, by being diſturbed, have produced the *returning ſtroke*.

“ To this objection (plauſible as at firſt ſight it may appear) I conceive I have given a complete answer in my Principles of Electricity, from ſection 337 to ſection 347 incluſively, and alſo from ſection 592 to ſection 606 incluſively; but it may not be improper to add a few words to what I have already ſaid upon that part of the ſubject.

“ No legitimate concluſion can be drawn from premiſes that are not proved; therefore, no perſon can legitimately conclude, that



that the force of a returning stroke must always be weak, when produced by the disturbed electrical fluid of a man's body, by reason that a man's body contains but a small quantity of electricity: for it has never been proved, that a man's body does contain only a *small* quantity of electrical fluid; neither is there the smallest reason to believe such an hypothesis, which appears, on many accounts, to be completely erroneous. And, if that hypothesis be erroneous, the objection to the strength of an electrical returning stroke remains perfectly unsupported by argument.

“ When a body is said to be *plus* or *positive*, it simply means, that the body contains *more* electricity than it does in its un-electrified, that is to say, natural state; but does not signify, that such body is completely saturated with electricity. In like manner, when a body is said to be *minus* or *negative*, it only signifies, that the body contains *less* than its natural share of electricity; but does not imply, that such body is *completely exhausted* of the electricity which it contains in its natural state.

“ Now,

“ Now, the strength of natural electricity is so immense, when compared to the very weak effects of our largest and best contrived electrical machines, that I conceive, that we cannot, by means of artificial electricity, expel, from a man’s body, the thousandth (or perhaps even the ten thousandth) part of the electrical fluid which it contains, when in its natural state.”

The importance of the subject, and the singularity of the effects produced by the above related thunder-storm, have induced me to dwell longer upon it than I at first intended. Lord Stanhope’s hypothesis is ingeniously adapted to the explanation of the phenomena; yet as many of the data, upon which that explanation depends, are mere suppositions, such as the situation of the clouds, &c. we can only consider it as a probable hypothesis, but not as a matter of demonstration.—I shall beg leave to add a few remarks on the general subject of thunder-storms.

Whoever considers the immense size of the electrified clouds in thunder-storms, and

the vast quantity of electrical matter discharged by them, will easily allow, that the experiments performed with our electrical machines can give us but a very inadequate idea of the effects produced by the electricity of the clouds: and though we derive considerable instruction from the use of our machines, yet we should never omit any opportunity of taking particular notice of the natural phenomena, since it is from them alone that we can be enabled to acquire adequate ideas of the natural operations, and to form such rules as are necessary either to judge of the effects, or to prevent those accidents, to which we are naturally exposed in thunder-storms.

It is well known, that when a large electrified cloud is near, the electrometers in the open air are powerfully affected by it; but my reader may rest assured, that from my own observation, as well as from the testimony of other persons, when a large electrified cloud is over head, the electrometers within a room, even when the windows and doors are shut up, are affected by the electrical atmosphere of the cloud. And when clouds  
differently

differently electrified succeed each other, or after a clap of thunder, the change may be frequently observed by means of the indoor electrometers. For this purpose the electrometers must not be inclosed in bottles, and as in that case gold leaf electrometers cannot be used, it becomes necessary to make those with cork-balls in a very nice manner. They answer rather better when they are made so that the cork-balls, when not electrified, do not actually touch, but remain at a very small distance from each other; for when the cork-balls are in actual contact, a degree of adhesion takes place, which is not easily overcome by very small quantities of electricity.

With respect to strokes of lightning, I have been assured by more than one eye-witness, that sometimes the lightning, in striking a building, a tree, or other object, assumes the form of a brush, and its branches seem to strike several points at once\*. This phenomenon may be imitated by presenting

\* Its more usual appearance is certainly of a single body or ball of fire.

a conducting body to the negative conductor of an electrical machine. By this observation we are enabled to understand, why sometimes a building, which is furnished with a conductor, happens to be struck by the lightning in some other part besides the conductor.

In such cases the damage is never very material; for first, the principal stroke is received and carried away by the conductor; and, secondly, that branch of the lightning, which falls on some other part of the building, soon finds its way to those conducting bodies that are connected with the principal conductor.

This observation does by no means invalidate the use of conductors for the defence of buildings from the effects of lightning; but only shews the necessity of erecting more conductors than one upon buildings that are not very small, and likewise the precaution of connecting together, and with the conductor, all the prominent metallic bodies which may happen to be on the top of a building.

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OF THE ACTION OF  
ELECTRICITY  
ON THE VEGETABLE KINGDOM

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**I**T has frequently happened, in almost every branch of natural philosophy, that a considerable deal of labour has been necessarily exerted to eradicate wrong notions, which had been introduced by artifice or by accident, and had afterwards established their reputation on the long acquiescence of the philosophical world. The history of Electricity has furnished several instances of this observation; and to that number one more will be added in this section.

That electricity promotes vegetation is a proposition, which was admitted many years ago, and has at various times received apparent confirmation from the publication  
of



of experiments and observations made either accidentally, or expressly for the purpose. The extensive influence of electricity in nature, its constant presence in the atmosphere, and its being generally found in rain, hail, snow, &c. corroborated the probability of its being either absolutely necessary or useful to vegetation; but Dr. Ingen-Houfz's attentive examination of the subject, and the result of his numerous experiments, have shewn the fallacy of the proposition, by having exposed the insufficiency of the experiments upon which it was established \*.

Previous to the publication of Dr. Ingen-Houfz's experiments and observations it was generally believed, that such plants as were kept in a state of constant electrization, would grow faster, and appeared more luxuriant than others of the same sort, which were treated in every respect like them, except that the latter were not elec-

\* See Dr. Ingen-Houfz's two letters to Mr. Molitor, published in the *Journal de Physique* for February 1786, and May 1788.

trified. It had been said, that certain plants which were situated near a conductor of lightning, grew to an uncommon size in consequence of that situation \*.—It had been likewise asserted, that some plants which were situated under some horizontal conductors or wires, such as are fixed for the purpose of exploring the daily electricity of the atmosphere, appeared sickly, and produced little fruit, as if the conductor robbed them of the vivifying electrical principle which they received from the air; and that after removing those wires, the plants soon became as healthy and as luxuriant as the rest †.

Dr. Ingen-Housz endeavoured to verify those assertions by means of accurate experiments, but the result proved, that in none of the above-mentioned cases the vegetation of plants appeared to be in the least influenced, either by the natural or artificial electricity. He exposed growing plants, and the seeds of plants, in earthen pots or

\* See the Abbé Bertholon's *Electricité des Météores*.

† See Giardini's *Dissertation de influxu electricitatis atmospherice in vegetantia*,

furnished with proper nourishment, to the constant action of electricity for a considerable time ; and, for the sake of comparison, kept other plants, and other seeds of similar plants, in the same situation, &c. but out of the influence of electricity. He found that the vegetation of the latter, did not appear to be either more or less forward, than that of the former. The experiments were repeated on different plants, and with various dispositions of the apparatus ; but the result was constantly the same, viz. that, whether they were electrified or not, and whether they were electrified positively or negatively, they grew with equal quickness and strength, provided every other circumstance remained unaltered \*.

\* “ Je faisois quelquefois ces sortes d’expériences, en appliquant aux plantes une électricité très-foible, & d’autres fois beaucoup plus forte, sans que j’ai jamais pu observer que les plantes exposées à un degré quelconque d’électricité, ayent prospéré plus que celles qui n’étoient pas électrisées du tout. Il m’a même paru plus d’une fois, que celles qui ne l’avoient été électrisées, étoient un peu moins avancées que les autres qui ne l’avoient pas été du tout.”

In the course of those experiments, the Doctor observed the extensive influence of light on vegetation, and found it to be so great, that he very justly attributes the mistakes of former experimenters, principally, to the neglect of that circumstance. That growing plants are in want of light, has been long known, but Dr. Ingen-Houfz found, that if adult plants wanted light, the germination of seeds is so far from being assisted by that element, that it is considerably retarded by the presence of it \*.

With

\* “ Non content de ces experiences, j’en fis d’autres, infiniment plus concluantes, en semant des grains de moutarde & de cresson sur les plus grands plats de fayence que je pouvois trouver, couverts de papier brouillard, & arrosés continuellement rempli d’eau. Chacun de ces plats étoit parsemé de plus de mille graines. Je tenois les plats électrisés nuit & jour, de la maniere que M. Schwankhard à decrite dans la lettre citée à M. Ehrmann, & que je m’abstiendrai de répéter ici, afin de ne pas grossir inutilement le mémoire. La végétation de ces espèces de petites forets, étoit toujours plus ou moins précoce, à mesure du plus ou moins de lumière que les plantes recevoient, & l’électricité ne contribuoit absolument en rien à les faire croître plus promptement.”

“ J’ai

With respect to the other assertions, viz. of the vegetation being promoted by the vicinity of a vertical conductor, and of being checked by the horizontal wires, the observations must have been made in a very inaccurate manner ; for, in the first place, by the very construction of a vertical conductor, such as is usually erected for the defence of buildings, the air about it, instead of being more electrified, is likely to be, in a great measure, deprived of its electricity by the conductor itself ; and as for the horizontal conductor, or exploring wire, the electricity of the air is so little absorbed by them, that on account of their being insulated at both ends, I do not think that an atmospherical electrometer would be less affected within a yard distance from the

“ J’ai observé que la lumière du soleil, si salutaire aux plantes adultes, est très-nuisible au developpement des semences, & à l’accroissement des plantes très-jeunes. C’est pourquoi les graines de moutarde, de cresson, & probablement de toute autre plante, se développent plutôt étant placées au fond d’une chambre, que lorsqu’on les met près des fenestres, & c’est probablement faute de cette attention, qu’on a porté jusqu’à présent un jugement erroné sur la cause de l’accroissement subit des plantes électrisées.”

conductor, than it would at any other distance from it. But Dr. Ingen-Houfz, in order to remove every possible difficulty, had several conductors situated vertically near some plants, and had others stretched horizontally over other plants. They were left in that situation for a considerable time, but no particular effect ensued, for the plants near the conductors appeared as vigorous, as others of the same species situated at a great distance from the conductors.

Amongst the other vegeto-electrical curiosities, it was said, some time ago, that the motion of the sensitive plant (*Mimosa*) was an electrical phenomenon, and that it would shut up its leaves, &c. when it was struck with some conductor of electricity; but that if struck with an electric, no contraction would ensue. My reader may rest assured, that from my own experience, as well as from that of many other persons, the asserted fact is by no means true. Whether that plant be struck with a conductor, as the human hand, a piece of metal, &c. or with a non-conductor, as glass, sealing-wax, and the like, the contractions take place



place without any apparent difference, provided the strength of the stroke, and every other circumstance, remains unaltered. Whether the plant be insulated or not, is likewise immaterial. Imagining that the singular property of that plant might be something analogous to the contractile power of animal bodies, which goes under the name of Animal Electricity, I applied different metals to the stock and leaves of a plant of that species, but I could not observe any particular phenomenon ; for when the plant was touched with the metals very gently, no contraction took place, and when struck with a certain degree of violence, then it would contract, whether it was touched with one metal, or two metals, or with any other body. In short, it seems that the peculiar contractile property of the sensitive plant has nothing to do with electricity. It must not, however, be understood, that electric shocks, or strong sparks, have no effect upon it, for by the action of those powers the plant is caused to contract its leaves very powerfully ; but this does not shew that the contraction of the plant is an electrical phenomenon, since the elec-

tric shocks or sparks stimulate the plant as much, or more, than the contact of other bodies, as they are applied in the usual manner.

I shall conclude this section, with mentioning the very easy and quick manner in which the plant, called *Balsam* (*Impatiens*) is killed by means of electricity. The plants of that genus are not remarkably delicate; they grow easily, their stocks and branches are thick in proportion to the size of the plant, and they bear the inclemency of the weather tolerably well; yet a very small electric shock sent through the stem of a balsam, is sufficient to deprive it of life. A few minutes after the passage of the shock, the plant will droop its head, the leaves and branches become flaccid, and, in short, its vegetation is quite destroyed. I have, indeed, known some plants of that species which have revived after a day or two; but that effect seldom takes place. A small Leyden phial, such as may contain six or eight square inches of coated surface, is sufficient for this purpose; and it may even be effected by means of strong sparks from

the prime conductor of a large electrical machine.

In this experiment, neither the internal vessels, nor any other part of the plant, appear to be injured, and indeed the size of the plant, and the inconsiderable strength of the shock which is used, are such as not to indicate the possibility of the vessels being burst, or of the vegetable organization suffering any other material derangement; it would, therefore, be useful to investigate the immediate cause which occasions the death of the plant.

Having subjected several other plants to the action of electric sparks and shocks from Leyden phials, I have not found any that can be so materially hurt by an electric power, so small in proportion to its size, as that which is sufficient to destroy the vegetation of a balsam.

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EXPERIMENTS AND OBSERVATIONS  
CONCERNING THE  
EFFECTS OF ELECTRICITY  
ON METALLIC SUBSTANCES.

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**I**N addition to the various effects of electricity on metallic bodies, which are contained in the preceding part of this treatise, I shall, in this section, collect together several other scattered materials of the same nature, as they may be of considerable assistance to those who are willing to follow the investigation of this curious and interesting subject.

In melting platina by means of electric shocks, I find a great difference in the results of the experiments, which seem principally to arise from the various states of that extraordinary

extraordinary substance. Having procured several specimens of platina in grains, I submitted them to the action of electric shocks, in the following manner: A groove was made, by means of an ivory paper-cutter, on a flat piece of wax. The depth of the groove might be about one-tenth of an inch. Two brass wires were placed at the extremities of this groove, so that their ends came within half an inch of each other, and in this space the grains of platina were laid one after the other, and care was taken that they might just touch each other; but no more grains of platina were used, than were necessary to complete the communication between the two wires. After this, the two wires were made to communicate with the inside and outside coatings of the charged electrical battery, and thus the shock was sent through the platina. The grains of metal were by this means partially, but evidently, fused, so that sometimes two or more of them adhered to each other.

As the grains of the metal, when promiscuously taken, were sometimes large, and at other times small, one might have  
 expected

expected that the small grains would have been fused easier than the large ones, but this was by no means always the case; some of the small grains were so refractory, as not to shew any marks of fusion when examined with the microscope, even after having suffered several electric shocks. On the other hand, some of the large grains were frequently agglutinated together by a moderate shock. Upon the whole, it seems that the whiter grains are fused more easily than those of a dark grey colour.

When the grains were heaped together in the groove without any distinction, and as strong a shock as the battery could afford was sent through, the marks of fusion were more evident than in the other case; but this effect seemed to depend on the small particles of gold and of iron that were intermixed with the platina; for when the experiment was tried with specimens of platina that were more pure and free from heterogeneous particles, the marks of fusion were less manifest. After this observation one would naturally imagine, that the purer the platina is, the more difficult it will be  
to



to fuse it by means of electric shocks; but I find that filings of malleable platina, treated in the above-mentioned manner, are more easily fused than the natural grains. This platina had been rendered malleable at Paris a few years ago by a Mr. Janeti, and it was sold as very pure platina, unmixed with any other metal.

The degree of fusibility of different metallic substances, when exposed to the action of electric shocks, is by no means the same as that which takes place in chemical furnaces, which shews that the electric fluid is not the same thing as the element of fire; the effects, in general, seeming to be proportionate to the degree of resistance which it meets with in its passage: but with respect to metals, the degree of fusibility which they shew in this way, seems to be in the compound proportion of that resistance which each particular metal offers to the passage of the electric fluid, and of its natural degree of fusibility when exposed to a common fire.

Many

Many persons have attempted to determine the degree of fusibility of metals by the action of electricity; but no one seems to have succeeded so well as Mr. Van Marum, who, for this purpose, employed the famous electrical machine belonging to the Museum of Teyler (which has been described in the second volume of this treatise) and an electrical battery, which contained 225 square feet of coated surface. Mr. Van Marum had wires of different metals drawn of the same diameter, which was equal to one thirty-second of an inch, and by exposing equal lengths of them successively to the above-mentioned battery, which was charged equally high in every experiment, found, that of the leaden wire 120 inches were melted, of tin wire the like quantity, of iron wire five inches were fused, of gold wire three inches and a half, and of the silver wire, or brass, or copper, a quarter of an inch only was melted, which shews a pretty good estimate of their fusibility by the action of electricity\*.

From

\* The degrees of fusibility of various metallic substances when exposed to a chemical fire, or rather of the degrees

From those and some other experiments made with the same apparatus, Mr. Van Marum deduces the following conclusions, viz. that lead is the worst, and, upon the whole, copper is the most eligible metal for the construction of a conductor of lightning.

That he could not possibly determine the proportion between the lengths and dia-

degrees of heat, which are necessary to fuse them, as calculated by the Academicians of Dijon, are as follow :

Tin	- -	170° of Rheaumur's thermometrical scale.
Lead	- -	230.
Silver	-	450.
Gold	- -	563.
Copper	-	630.
Iron	- -	696.

According to the experiments of the late Mr. Wedgwood, F. R. S. the degrees of heat, which are necessary to melt metallic bodies, are as follow :

Brass	- - -	3,807° of Fahrenheit's thermometrical scale.
Swedish copper	-	4,587.
Fine silver	- -	4,717.
Fine gold	- -	5,237.
Cast iron	- -	17,977.

See the Phil. Transf. Vol. LXXII.

meters

meters of metallic wires, that could be melted by the power of the Teylerian machine.

That iron, tin, and copper, were melted into globules, but this was not the case with the other metals.

That the metallic globules were sometimes thrown to the distance of thirty feet, and upwards.

That the globules of tin remained red hot for about eight or ten seconds; and

That when the wires were very long, the fusion was but partial.

Mr. Van Marum had the curiosity of trying whether the metallic wires could be fused and calcined in water, viz. by sending the charge of the battery through them whilst standing under water, and he succeeded whenever he used the eighth part of that length of wire, which would have been calcined if the experiment had been performed in air.

The

The easiness with which iron and steel are fused by means of electricity, and the violence with which small particles of them burn, renders them peculiarly useful in entertaining experiments. The filaments produced in turning of steel are so very readily ignited by an electric shock, that such of them as are very fine, may be burned by means of a six-ounce coated phial, or even a smaller one. They may be also burned by a single spark from a large prime conductor. If one of those filaments or shavings, which may be frequently procured at the opticians shops, be put through a cartridge, the gun-powder may, in that case, be fired by means of a very small electric shock.

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NOTES AND ADDITIONSTO VARIOUS PARTS OF THIS TREATISE.

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## N° I.

**A**N experiment is described in page 324 of Vol. I. which shews that hot air is a conductor of electricity. From the manner in which that experiment is performed, one may be led to suspect that the effluvia which proceed from red-hot iron may contribute towards rendering the air a conductor. It appears likewise, that the air must be heated to a considerable degree in order to become a conductor. Without entering at present into a minute discussion of those particulars, I shall here only subjoin an experiment of Mr. READ, by which he means to prove that hot air is not a conductor.

“IT



“ IT has been also commonly said  
 “ that hot air conducts electricity. With  
 “ a view to ascertain this matter, the fol-  
 “ lowing experiments were made: To one  
 “ end of a long piece of wood (which serv-  
 “ ed as a handle) was fixed a glass rod fif-  
 “ teen inches long; to the remote end of  
 “ the glass was fixed a pith-ball electrome-  
 “ ter. Having electrified the balls, I held  
 “ them by the wood handle, and projected  
 “ them into a large oven, immediately after  
 “ the fire was drawn out of it; the conse-  
 “ quence was, that when I performed the  
 “ operation slowly, the balls lost their elec-  
 “ tricity; but that when done quick, with  
 “ as little delay as possible, their electric  
 “ charge was not diminished. The loss of  
 “ electricity, in the first case, was found to  
 “ have escaped along the glass into the  
 “ wooden handle, and so to the earth, ow-  
 “ ing to the great heat the glass rod had  
 “ acquired, by which it became a conductor  
 “ of the fluid, for until it had cooled a lit-  
 “ tle the balls could not be charged again.

“ I shall lay before the reader one cir-  
 “ cumstance more, because it may tend to

“ throw light on what degree of heat the  
 “ oven was in at the time the observations  
 “ were made. The baker having pointed  
 “ out to me the hottest part of the oven ;  
 “ with a quick motion in and out, I plung-  
 “ ed the electrified balls into that part of  
 “ it, by which one thread and ball was  
 “ burned off, but the remaining ball shew-  
 “ ed that it still retained its electric charge,  
 “ because it was strongly attracted on the  
 “ approach of my finger \*.”

The focus of a burning lens or mirrour is  
 not a conductor of electricity. Let a wire  
 that proceeds from the outside of a charged  
 Leyden phial, come within an inch of the  
 knob of the jar, or, in short, so as to be very  
 little farther from it than the striking dis-  
 tance. Let then the focus of the solar rays  
 that are collected by a lens or mirrour, fall  
 midway between the knob of the phial and  
 the wire which proceeds from its outside,  
 and it will be found, that the charge of the  
 phial is not thereby dissipated ; whereas, if

\* Read's Summary View of Spontaneous Electricity,  
 p. 8 and 9.

the flame of a candle, or any other conductor of electricity be interposed between the knob and the wire, the discharge will immediately take place. This experiment seems to corroborate the supposition, that the rays of the sun, or of light in general, have no heat in themselves, but only extricate the elementary heat from such bodies as they happen to fall upon, provided those bodies obstruct their course, and are not transparent.

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## N° II.

*The following paragraphs contain additional properties of charged electrics.*

“ IT is unnecessary to insist upon what is called the equilibrium of an electrical charge, because Dr. FRANKLIN has admirably explained it according to his hypothesis. But there is another important particular, which has been almost entirely overlooked, namely, the uncompensated electricity, which is as essential to the charge

as that which is in equilibrio. Whenever a jar is charged, the greatest part of the electricity becomes latent on account of the compensation; but there is a certain proportion which remains on the insulated side, and exerts its force to prevent the electricity from returning to the outer surface. In moderate intensities this will explode, and carry the charge with it to distances which are in proportion to the quantity of the charge itself; but in greater intensities the distances greatly exceed that proportion. With glasses of different thicknesses, this intensity, as measured by the explosive spark, is as the thickness when the charges are equal, as Mr. Cavendish has determined, and I find likewise by experiments with thin substances; but when the thicknesses are greater, it increases in a higher proportion, as is found by the explosion which takes place between the electrophore and its plate, as well as by other experiments.

“ This uncompensated part of the charge (which is commonly in proportion to the quantity of latent or compensated electricity, or to the distance at which it exerts its  
 \* action)

action) was found to be greatly increased when a series of jars were made to charge each other. If a jar be insulated, and made to explode by LANE's electrometer at a determinate number of turns, and another jar be then connected with its external coating, so as to become charged by that means, the explosion from the outside of the last, to the inside of the first, will take place at the electrometer (unaltered) with much fewer turns. Or if the electrometer be altered till the explosion takes place at the original number, the distance will be much greater than before. Hence we see, that the intensity of the uncompensated part must be greater when there is a greater charge to be maintained, whether it be on one surface only, or on two surfaces successively connected.

“ It is evident, that the breaking of jars is not effected by any attraction between the electricities which form the charge, but by this necessary surplus; for thicker glasses require much less electricity to produce an intensity which breaks them, than thinner do; and I found a piece of Muscovy talc,

one-hundredth of an inch thick, to bear a charge consisting of ten times the quantity of electricity, which was sufficient to have charged an equal surface of common glass, so as to break it; but the intensity of the very dense charge on the talc was so low as to afford an explosion of no more than about one-tenth of an inch, while that of the glass jar it was compared with exploded through about five inches.

“ The perforation of glass by the long spark, or by the spark through oil or cement, seems to depend on the very great intensity of the electricity which has not time to diffuse itself, but charges a minute part of the surface very high.

“ Muscovy talc being a very perfect non-conductor, and capable of being divided into plates of less thickness than one two-hundredth part of an inch; I made many experiments with it, which are too numerous to enter into this paper. In consequence of its great capacity it gives very strong shocks. Contrary to the assertion of BECCARIA, I found that its laminæ are naturally



turally in strong opposite states of electricity, and flash to each other when torn asunder in the dark. A large piece being split in two, the parts were found to be in opposite states. The greatest care was taken in these experiments to avoid friction, and to use such pieces as had never been excited, nor brought near the machine \*."

It is well known, that after the discharge of the Leyden phial, a residuum of electricity always remains on the glass whether coated or not. This residuum is very seldom to be perceived immediately after the discharge ; but it becomes manifest a certain time after, continues increasing to a certain degree, and then decreases until it entirely vanishes. Mr. Read has observed, that the accumulation of this residuum is accelerated by heat. " I was," *says he*, " shewing a gentleman the power and utility of Mr. VOLTA's condenser of electricity ; in order to which, a Leyden bottle was charged, and then discharged, by touching

\* Mr. Nicholson's Experiments and Observations on Electricity.—Phil. Trans. Vol. LXXIX.

its coated sides with a discharging rod. It now appeared to be deprived of all its electricity; yet if I held it by the coating, and touched with the knob of it the metal plate of the condenser (placed upon an imperfect conductor) and held them in contact a few seconds of time, then removed the bottle, and took up the metal plate by its insulating handle, and presented it to a sensible electrometer, the electricity was strong enough to enable me to ascertain its quality. But these beautiful effects diminished by repetition; and I repeated the experiment until no electric signs appeared in the condenser. However, while we were talking the matter over, I still held the bottle in my hand, and after a long interval of time, from our last trial, I repeated the operation once more; and, to our astonishment, we found the bottle still strongly electrical. This new revival of the electricity we could attribute to nothing but the diffusive warmth of my hand, acting on the electricity of the bottle. This set us to work again, and to try the bottle by turns in a warm and cold state, and we found that our conjecture of the  
the

the cause was confirmed by repeated trials \*."

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### N° III.

THE amalgam of zinc and quicksilver, described in page 142 of vol. I. is apt to become very hard, especially when left untouched for some time; this may be partly prevented by mixing it with a little grease, as tallow, or mutton-suet, and a very small portion of finely powdered whitening. The proper way of preparing this amalgam is the following: Let the quicksilver be heated to about the degree of boiling water, and let the zinc be melted in a crucible or iron ladle. Pour the heated quicksilver into a wooden box, and immediately after pour the melted zinc in it. Then shut the box, and shake it for about half a minute. After this you must wait until the amalgam is quite cold, or nearly

\* Read's Summary View of the Spontaneous Electricity, p. 16.

so, and then you may mix the grease with it by trituration.—If the melted zinc be poured into the quicksilver when cold, a very small portion of the former will be amalgamed, the rest remaining in lumps of different sizes.

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#### Nº IV.

#### *Of the suspected spontaneous production of Electricity from living animals.*

SEVERAL persons have suspected, and some have even asserted their having discovered, that living animals generally yield some electricity, which, though mostly positive, is however sometimes negative. Those suspicions and assertions, joined to the recent discovery of that property of animal bodies which has been called *animal electricity*, induced me to try whether any such spontaneous production of electricity could be ascertained or disproved; and with this view I made some experiments, the result of which seemed to shew, that no electricity

was

was produced by any natural process in the animal œconomy. The method which I adopted was as follows: I insulated either myself or some other person, and after a certain time I examined, by means of an electrometer, whether any electricity was produced. The electrometer was generally electrified, in a small degree, either positively or negatively; but it seemed to me that this electricity was either produced from some accidental friction, or it was communicated by the surrounding air; for when the electricity of the atmosphere was very weak, and the insulated person avoided every sort of movement as much as it was possible, the electrometer shewed no signs of electricity.

In those experiments I did not think proper to use the multiplier, considering, that as a small quantity of electricity generally adheres to a person's cloths, to the hair, &c. one might be misled by it, so as to imagine that this electricity was produced by the natural functions, or respiration, of the animal body itself.

## N° V.

*Observations and experiments relative to the Electricity produced by evaporation.—See page 267 and 268 of vol. II.*

MR. VOLTA's discovery of the electricity which is obtained from the evaporation of water, and such other solid or fluid bodies as can be reduced into smoke or vapour ; besides its affording an easy explanation of the origin of the electricity in the clouds, &c. seemed to point out a general law of nature, viz. that the capacity of water, and other fluids, for holding the electric fluid, was increased by those bodies being expanded into the state of elastic vapour, and was diminished by those bodies being condensed into their other fluid form. Therefore, in the first case, by imbibing an additional quantity of electric fluid, they would leave the bodies, that happened to be in contact with them, in a negative state ; and, in the second case, by giving out a quantity of electric fluid, they would leave the bodies, that happened to be in contact with them, in a positive state.

All



All the experiments made on evaporation for some years after this discovery, were attended with results conformable to the above-mentioned general law ; but two remarkable exceptions have of late been discovered, which, besides their contradicting the said law, seem to point out a more intimate connection between the electric fluid and other bodies. The first of those exceptions was discovered and published three years ago, by a learned professor of the Academy of Mantua ; the second was very lately discovered by myself.

The Mantuan Professor observed, that when water was evaporated by being put in contact with a red-hot piece of rusty iron, it would leave the iron electrified positively ; whereas when the experiment was tried with a clean piece of iron, the electricity acquired by the metal would be of the negative kind \*.

When

\* *Parvam et tenuem laminam ferream, quatuor aut quinque pollicum extensam, et ferrugine undique tectam, data opera selegi, hanc intra carbones tandiu detinui, quandiu candens fieret caute agens ne rubigo perderetur,*

When I first attempted to repeat this curious experiment, the result did by no means answer my expectations; the electricity, which was produced being of the negative, and not of the positive kind; but observing that sometimes no sensible degree of

tur, hanc projeci intra vas æneum tanta aqua plenum, quanta sufficeret ad totam laminam tegendam; ex extinctione lamellæ ebullitio, et evaporatio maxima orta est, et investigata electricitas subito deprehensa est positiva ad lineas duas, et ultra, quæ paulatim minuebatur, prout minuebatur calor, ebullitio, et evaporatio, et istis cessantibus cessavit et ipsa. Alio frustulo ferreo simili similiterque ferrugineo, et fortasse a primo separato, et secto expertus sum, et positivam etiam obtinui duarum linearum. Præterea iisdem frustulis iterum usus sum, et nullam amplius habui positivam electricitatem, amiserunt ipsa omnem ferruginem suam a primis experimentis; tentavi per alia frustula ferruginea, æque positivam dederunt; potuit ne aliquid præstare data ferrugo, quæ maxima erat, et ubique per universam superficiem dispersa ad reddendam electricitatem positivam, quæ cæteroquin negativa esse debebat ob nimiam et repentinam evaporationem. Josephi Gardinii Dissertatio de Electrici Ignis Natura, p. 124.

The same gentleman observed, that in other experiments of the same nature, the positive electricity was also produced; but that effect was not constant, nor have I been able to obtain the same result.—See the above quoted work, p. 126, 129, 133.

electricity

electricity was produced, though the evaporation was very quick and copious, I began to suspect that the iron, which I had employed, was not sufficiently covered with rust, in consequence of which two opposite states of electricity might possibly be produced, viz. the negative from the clean, and the positive from the rusty, part of the iron: which two opposite states, by counteracting each other, would leave the iron un-electrified. After various repetitions of this experiment, in which either the red-hot iron was thrown into the insulated water, or the water was poured upon the red-hot and insulated iron, I found that this was actually the case.

I procured some old iron nails, which had remained exposed to the atmosphere for several years, and of course had contracted a very thick coat of rust; and on performing the experiment with them, I obtained positive electricity, agreeably to the assertion of the above-mentioned gentleman. The same nail very seldom would answer for more than one experiment; for the action of the fire and of the water generally removed a great

deal of the rust, and exhibited the naked metal, which would afterwards acquire the negative electricity. Here follows the manner of performing this remarkable experiment.

Insulate a metallic or earthen plate, and pour a small quantity of water in it, and let a sensible electrometer be connected with the water; then drop a red-hot piece of iron into the plate, and it will be found, that, if very rusty iron be used, the electrometer will be opened with positive electricity; if the iron be clean, or free from rust, the electrometer will acquire the negative electricity; and lastly, if the iron be partially rusty, the electrometer will acquire little or no electricity, though in every case the evaporation may be equally quick and copious.

The other exception of the above-mentioned general law is shewn by means of red-hot glass, which I chanced to discover very lately. The various degrees of electric power that are produced by the evaporation of water from different substances induced  
me

me to diversify the experiments as much as I could, in order to discover, if possible, the reason why those different effects took place when the evaporation seemed to be equally quick and copious. Amongst other substances, I tried glass, and found that it generally produced little or no electricity. The water was sometimes poured upon the hot glass, but in general the hot glass was dropped into the insulated water, which was contained in a tin cup. However, the difference of effect was found not to be occasioned by those two different modes of proceeding. Having repeated this experiment a great many times, I at last found, that the effect depended on the different nature of the glass. If white and clean flint glass be made red-hot, and in that state be dropped into the vessel of water, a quick evaporation will ensue, and the vessel is electrified positively. If the flint glass be not very clear, there will not be any electricity generated by the evaporation, &c. And lastly, if the experiment be tried with more impure glass, as the glass of which wine bottles are made, the negative electricity will be produced.



In performing this experiment, it is necessary to take care that no pieces of coal adhere to the glass, which will frequently happen when a piece of glass is heated in a common fire ; for in that case negative electricity will be produced by the evaporation, though the best flint glass be used.

It has frequently happened, in the course of my experiments, that no electricity whatever has been produced by the evaporation of water from certain substances ; however, as in those cases the evaporation was not very copious, I attributed the deficiency of electricity to the weakness of the evaporation. But a very remarkable instance of this sort is mentioned in the dissertation of the above-mentioned ingenious Professor \*. He flaked 25 pounds weight of quicklime with a sufficient quantity of water, and though a very copious evaporation took place, yet it was not attended with any electricity. Should any person suspect, that the deficiency of electricity in this experiment was owing to the want of burning coals or

\* Page 131.



actual fire, he should consider, that in other similar processes electricity is produced without any actual fire; thus the evaporation, which is occasioned by the effervescence of iron filings in diluted vitriolic acid, produces negative electricity.

After a careful examination of the above-mentioned experiments, the origin of the electricity, which is observed in the evaporation of water and other evaporable substances, whether solid or fluid, seems not to be reconcileable to the general law already noticed, nor can I form any plausible theory that can be sufficient to account for all the phenomena. If the production of electricity in those experiments depended upon the increased or diminished capacity of water for holding the electric fluid, it should seem to be immaterial whether the water be evaporated in one way or in another, provided the evaporation be made with equal quickness and in equal quantities. Were it not known that glass or iron made red-hot produces no electricity in cooling, we might suspect, that the electricity, which is produced by the evaporation of water, may be

counteracted by the contrary electricity, which is produced by the cooling of glass or iron; but it has been observed by several ingenious persons, that red-hot glass and red-hot iron produce no electricity whatever when suffered to cool upon insulated stands.

Considering the circumstances which might be common to white flint glass and to calcined iron, it occurred to me, that as they were both destitute of the power of inflammability, or of phlogiston\*, therefore other substances, that were in the same predicament, might produce the like effect, viz. might acquire the positive electricity from the evaporation of water thrown upon them after being made very hot. With this view I tried a piece of a tobacco-pipe made hot; I also tried another piece of earthen-ware of a very refractory nature,

\* Having not mentioned any of the new chemical names in the course of this work, it will be useless in this place to take any notice of the antiphlogistic theory; but it will be sufficient to observe, that the remark concerning the nature of diverse substances, &c. is applicable to either theory.

and

and likewise tried flints ; but the evaporation of water thrown upon those substances always produced the negative electricity. The elucidation therefore of those phenomena must be deferred until other facts are ascertained by future experiments. For the present we can only say, that the electricity, which is manifested in the evaporation, seems to depend on some other circumstance besides the mere expansion of water or other fluids into the form of elastic vapour.

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## N° VI.

*Description of the self-charging Leyden phial.*

TAKE a glass tube of about eighteen inches in length, and an inch, or an inch and a half, in diameter. It is immaterial whether one of its ends be closed or not. Coat the inside of it with tin-foil, but only from one open extremity of it to about as far as its middle; the other part, which re-

mains uncoated, we shall call the naked part of the instrument. Put a cork to the aperture of the coated end, and let a knobbed wire pass through the cork, and come in contact with the coating. The instrument being thus prepared, hold it in one hand by the naked part, and with the other hand clean and dry-rub the outside of the coated part of the tube; but after every three or four strokes you must remove the rubbing hand, and must touch the knob of the wire, and in so doing a little spark will be drawn from it. By this means the coated end of the tube will gradually acquire a charge, which may be increased to a considerable degree. If then you grasp the outside of the coated end of the tube with one hand, and touch the knob of the wire with the other hand, you will obtain a shock, &c.

In this experiment the coated part of the tube answers the double office of electrical machine and of Leyden phial; the naked part of it being only a sort of handle to hold the instrument by. The friction on the outside of the tube accumulates a  
 quantity

quantity of positive electricity upon it, and this electricity, in virtue of its sphere of action already explained in the first volume, forces out of the inside a quantity of electricity also positive. Then by taking the spark from the knob, this inside electricity, which is by the coating communicated to the knob through the wire, is removed, consequently the inside remains under-charged or negative, and of course the positive electricity of the outside comes closer to the surface of the glass, and begins to form the charge. By farther rubbing and taking the spark from the knob this charge is increased, &c.

Instead of a tube this instrument may be constructed with a pane of glass, in which case it will be rather simpler, but it cannot be managed so easily, nor of course can it be charged so high as the tube. A piece of tin-foil must be pasted in the middle of only one surface of the pane, leaving about two inches and a half or three inches of uncoated glass all round. This done, hold the glass by a corner, with the coated side from you, and with the other hand rub its  
uncoated



uncoated side, and take the spark from the tin-foil alternately, until you think that the glass may be sufficiently charged; then lay the glass with its uncoated side flat upon one open hand, and on touching the tin-foil with the other hand you will receive the shock.

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## N° VII.

*Of the decrease of the density of electricity in electrified atmospheres.*

FROM the analogy of other emanations that proceed from a centre, and are expanded in all directions, it might have naturally been concluded, that the intensities of electric atmospheres (by which we only mean the power of acting on other bodies) at various distances from the electrified bodies, are inversely as the squares of those distances. Thus heat, light, and the force of gravity decrease as the squares of those distances, or, which is the same thing, their intensities, are inversely as the squares of the distances from the centres from which



which they emanate ; so that at three times a certain distance they are nine times weaker, at five times that same distance they are twenty-five times weaker, and so on\*. But independent of this analogy, it has been proved by actual experiments, and by mathematical reasoning, which was deduced from those experiments, that such is the law which is followed by the intensities of electric atmospheres,

It has been shewn in the course of this work, that when an insulated conducting

\* The usual way of shewing the truth of this general law, is by demonstrating, that the surfaces of spheres are as the squares of their diameters, or the squares of their radii ; for if we conceive two spherical surfaces to be described at two given distances from a luminous centre, for instance, at the distances of one and two feet, it is evident, 1st, that the same rays of light which pass through the one must likewise pass through the other of those spheres ; 2dly, that as the second spherical surface is larger than the first, the rays must be proportionably more expanded at the second spherical surface than at the first ; but the spherical surfaces are as the squares of the distances, which squares are one and four ; therefore the rays of light, or of any other of the above-mentioned emanations, must be four times more expanded or less intense at two feet than they are at one foot distance from the centre.

body

body is brought within the sphere of action of an electrified body, the former acquires the two electricities on opposite sides: and that in some place called the *neutral point*, which lies between the two contrary electrified extremities, the insulated body remains in its natural state. Now Earl Stanhope first determined mathematically the situation of such neutral point in a cylindrical body properly situated, upon the supposition that the intensity of electric atmospheres decreased in the simple proportion; and likewise upon the supposition of its decreasing in the duplicate proportion (viz. as the square) of the distance. Then he proceeded to experiments, and by presenting an electrometer to various parts of the insulated cylindrical body, found that the place which neither attracted nor repelled the electrometer coincided with the neutral point which had been determined upon the supposition that the intensity of electric atmospheres decreased as the square of the distance \*.

\* See Lord Mahon's (which was Earl Stanhope's former title) *Principles of Electricity*, Part IV. V. and VI.

The same law of electric atmospheres has been also confirmed experimentally by other persons. Their method in general has been to examine, by means of scales and weights, or of similar instruments, the actual attractive force exerted towards a certain body by an electrified body placed at different distances.

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N° VIII.

*Farther uses to which the electric power may be applied, in addition to chap. the 9th of part I.*

THE attempts recently made to convey intelligence from one place to another at a great distance, with the utmost quickness, have induced me to publish the following experiments, which I made some years ago, and of which I should not have taken any farther notice, had it not been for the above-mentioned circumstance, which shewed that they may probably be of use for that and other purposes.

The

The object for which those experiments were performed, was to fire gun-powder, or other combustible matter, from a great distance, by means of electricity. At first I made a circuit with a very long brass wire, the two ends of which returned to the same place, whilst the middle of the wire stood at a great distance. In this middle an interruption was made, in which a cartridge of gun-powder mixed with steel filings was placed. Then, by applying a charged Leyden phial to the two extremities of the wire, (viz. by touching one wire with the knob of the phial, whilst the other was connected with the outside coating) the cartridge was fired. In this manner I could fire gun-powder from the distance of three hundred feet and upwards. But this method was subject to the following inconveniencies. As the wire was laid upon the ground, the experiment would answer only when the ground was very dry. It was found necessary to use a large jar for the purpose, viz. one which contained about three square feet and a half of coated surface; and lastly, it proved very troublesome to dispose the two wires between the cartridge and the

Leyden jar so as not to touch each other in some intermediate place; for when that happened the powder could not be fired, the charge naturally passing from one wire to the other at the place of their mutual contact.

In order to avoid the last-mentioned inconvenience, I tried the experiment with one wire. A brass wire of about a 50th part of an inch in diameter, and 200 feet long, was laid on the ground, and its extremity was inserted in the cartridge of gun-powder and steel filings. Another piece of the same sort of wire was likewise inserted with one end into the same cartridge, and its other end was thrust into the ground. Then, by applying the knob of the charged jar to that extremity of the long wire which was remote from the cartridge, whilst the outside of the jar communicated with the ground, I expected that the circuit would be formed partly through the wire and partly through the ground, and of course that the powder would be fired as in the preceding experiment. That the charge of the jar passed through the wire, and  
through



through the ground, was evidently proved by the powder being sometimes fired; but part of the charge was certainly dissipated before it reached the cartridge; for in this disposition of the apparatus a much greater charge was required to produce the desired effect, so that a battery of nine ordinary cylindrical jars would but just suffice when the ground was dry, and every other circumstance was favourable.

Considering that the management of such a battery as might be sufficient for this purpose could not be charged or moved about very easily, I attempted to use some more inflammable substance instead of gun-powder. Phosphorus was the substance which I tried next. The apparatus was disposed as in the last experiment, excepting that in the place of the cartridge a small piece of phosphorus was situated, into which the two extremities of the wires were thrust, so as to come within about a fiftieth part of an inch from each other.—In this case a smaller charge even than that which had been used in the first experiment was found sufficient to inflame the phosphorus; yet this method



was found subject to other inconveniences. Sometimes the phosphorus would be split without taking fire; at other times the charge would not pass through it; at least it shewed no signs of such passage, every thing remaining unaltered. It was also found necessary to try the experiment soon after situating the phosphorus in its proper place; for if three or more hours elapsed, either the softening or wasting of the phosphorus prevented the success.

I endeavoured to substitute other combustible substances; but after all I found none to answer either better or so well as inflammable air. After having discovered that this permanently elastic fluid was by far the readiest to be inflamed by means of electricity, I tried to improve the wire of communication, and after several attempts I at last constructed one, which might be laid upon dry or wet ground, and even partly through water.

Without detaining my reader any longer with the narration of the intermediate trials, I shall subjoin the description of the method

thod which I found to succeed best, and by which means gun-powder, or other combustibles, may be certainly fired from the distance of two or three hundred feet, and probably from a much greater distance without danger to the operator, and without any loss of time ; for the inflammation will take place at the moment that the knob of the charged jar is presented to the extremity of the long wire.

The inflammable air must be contained in a two or three ounce phial, such as is used for medicines, which must be prepared in the following manner : Turn the phial with the bottom upwards, and by striking the apex of the conical cavity with a pointed thick wire, a hole will be made in the bottom of the phial. In this hole a piece of thin wire must be cemented, so as to project about an inch within the phial, and its external part must be bent in the form of a hook. A very sound cork must be fitted to the mouth of the phial, and another piece of thin wire must be passed through it, so as to project within the phial, and to come with its extremity to the distance of about  
the

the fortieth part of an inch from the extremity of the wire that proceeds from the bottom. The external part of this wire in the cork should be about two or three feet in length. The phial being thus prepared, remove the cork from it, and place it inverted for about four or five seconds of time, over the mouth of another large phial full of inflammable air; then slip it off, and cork it up as fast as you can. By this means a certain quantity of inflammable air will be introduced, which, by mixing with the common air contained in the phial, will form a compound elastic fluid, which is so ready to take fire, that if the least electric spark be passed from wire to wire through the phial, the air will instantly explode, and the cork will be pushed out of it with violence.—The long wire of communication is prepared in the following manner :

A piece of annealed copper or brass wire, of about a fiftieth or fortieth part of an inch in diameter, being stretched from one side of a room to the other, heat it successively from one end to the other by means of the flame of a candle, or of a red-hot

piece of iron, and as you proceed with the candle, rub a lump of pitch over the heated part of the wire. When the wire has been thus covered with pitch, a slip of linen rag must be put round it, which can be easily made to adhere to the pitch, and over this rag another coat of pitch must be laid with a brush, the pitch being melted in a pipkin or other convenient vessel. This second layer of pitch must be covered with a slip of woollen cloth, which must be fastened by means of a needle and thread. Lastly, the cloth must be covered with a thick coat of oil paint, and when the paint is dry, the covered wire may be used for the experiment. In this manner many pieces of wire, each of about twenty or thirty feet in length, may be prepared, which may afterwards be joined together, so as to form one continued metallic communication ; but care must be taken to secure the places where the pieces are joined, which is most readily done by wrapping a piece of oil-silk over the painted cloth, round the two contiguous extremities, and binding it with thread. When a long wire has been thus made out of the various short pieces, let one end of it, purposely

posely left out beyond the above-mentioned envelopes, be formed into a ring, and to the other extremity adapt a small brass ball.

In order to perform the experiment, lay the wire upon the ground in any direction, fasten the hooked wire of the phial that contains the inflammable air to the ring at one end of it, and push the extremity of the wire that comes out of the cork of the said phial about an inch or two into the ground. Then, if you bring the knob of a charged Leyden phial in contact with the ball at the other extremity of the long wire, the inflammable air will explode immediately, and the cork will be pushed out of the neck of the phial.

For this purpose a Leyden phial, that contains about one square foot of coated surface, will be found to be quite sufficient, when the distance between the operator and the spot in which the inflammation is to take place does not exceed 200 feet. For greater distances it will be necessary to use Leyden jars of proportionably larger size.

Thus we have shewn how inflammable air may be fired ; but in order to set fire to gun-powder, the only addition which needs be made, is to surround the cork and neck of the phial with loose cotton, that has been previously filled with pounded gun-powder, from which a quick match may be continued, &c.; for when the cork is pushed out of the neck of the phial by the explosion of the inflammable air, a flame comes out which has power sufficient to set fire to the gun-powder in the cotton. I have sometimes put a small quantity of cotton rubbed in gun-powder into the phial of inflammable air, besides the cotton on the outside; but it is by no means necessary.

The only inconvenience which I have found to attend the above-described method is, that sometimes the very nature of the inflammable air in the phial suffers such an alteration as to loose its inflammability, in which case the experiment will not be attended with the desired effect. But this degeneration of the inflammable air does seldom, if ever, take place in less than two or three days, and sometimes not even in a fortnight ;



fortnight; so that one may be sure that the experiment will answer within 24 or 30 hours. And it is probable that it will answer within two or three days. It is immaterial whether the phial be left attached to the long wire on the ground all that time, or be fastened to it the moment before the time of the inflammation.

If this experiment be tried without any inflammable air in the phial, the only effect will be, that a spark will be seen between the two wires in the phial at the moment that the charged jar is applied to the knobbed end of the long wire. By sending a number of sparks at different intervals of time, according to a settled plan, any sort of intelligence might be conveyed instantaneously from the place in which the operator stands to the other place in which the phial is situated. With respect to the greatest distance to which such communication might be extended, I can only say, that I never tried the experiment with a wire of communication longer than about 250 feet; but from the results of those experi-

U 4. ments,

ments, and from the analogy of other facts, I am led to believe that the abovementioned sort of communication might be extended to two or three miles, and probably to a much greater distance.

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## N° IX.

*Preparation of the Electrical Amber Varnish.*

IT is well known that glass frequently becomes a conductor of electricity, in consequence of its being apt to attract moisture from the atmosphere; to avoid this inconvenience, the makers of philosophical instruments generally cover the glass legs and other parts of the electrical apparatus with a coat of some non-electric substance. Sealing-wax and amber varnish have been principally used for this purpose. The sealing-wax has been laid on the glass two different ways, viz. either by making the glass sufficiently hot, and then rubbing a piece of sealing wax upon it, or by first dissolving the sealing-wax in spirit of wine, and then lay-

ing it upon the glass with a hair pencil\*. The first of those methods is in my opinion the best of any, but it is not practicable with large pieces of glass; for, besides the danger of breaking the glass, when a large piece of that substance has acquired the degree of heat that is sufficient for the purpose, it will retain that heat so long as to dry the sealing-wax too much. With respect to the other method, when the sealing-wax is dissolved in very highly rectified spirits of wine, and the glass is previously warmed, it will answer tolerably well, but it is certainly inferior to the first method.

The electrical amber varnish, when properly prepared, and when carefully laid upon the glass, will preserve its insulating property full as well as the sealing-wax, which is applied by means of heat; but the preparation of this varnish requires a great deal of attention; for if it be not properly made, the glass will not be in the least improved

\* Some persons have used common spirit varnish mixed with a little vermilion; but this sort of covering serves more for ornament than for use, the glass being little if at all improved by it.

by it; I shall therefore be as particular as I can in describing the process, and in pointing out the causes which are most likely to prevent its success.

*Preparation of the Amber.*

Reduce some pieces of amber (the yellow amber is the best) into tolerably fine powder, and then melt it in an unglazed earthen vessel over a charcoal fire; \* when melted, pour it whilst fluid upon an iron plate, and as soon as it is become cold, it must be pounded and sifted through a very fine sieve.

*Process for making the Varnish.*

Half a pint of linseed oil, one ounce of *saccarum saturni* (sugar of lead) and one ounce and a half of litharge, must be set to boil in an iron pot over a charcoal fire. As soon as the oil has dissolved the two

\* It must not be expected that the amber will melt into as fluid a state as water or oil; for it will hardly acquire the fluidity of cold honey, and therefore it must not be kept upon the fire longer than may be necessary to produce that effect, otherwise it will be burned too much.

other

other ingredients, one ounce and a half of the prepared amber must be added, and then the whole must be left to boil to a proper degree of consistence. Lastly, it must be mixed with such a quantity of spirit of turpentine as will bring it to the consistence of olive oil.—I shall now subjoin the necessary precautions.

The capacity of the boiler should be at least four or five times greater than the bulk of materials, in order to allow for the swelling of the composition, which is very considerable towards the latter end. For the same reason the boiler must be constantly watched, and must be removed from the fire whenever the liquor comes near the top; for if any of it happen to run over, it will take fire immediately. The mixture should be stirred every two or three minutes, but towards the latter end, viz. after it has boiled for about two hours, it should be stirred oftener. An iron ladle is very fit for this purpose.

The consistency of the liquor shews when it has boiled sufficiently; for if at  
that

that time a drop of it be rubbed between two knife-blades, and the blades are afterwards separated, the varnish will stretch itself like a continued thread from one blade to the other. When this effect takes place, the pot may be removed from the fire, and left to cool, but before it becomes quite cold, the spirit of turpentine must be mixed with it. The best way of forming this mixture is to put the spirit of turpentine into a bason or pipkin, then to add one ladle full of the varnish at a time, and to stir it until it is quite dissolved in the spirit. The varnish may then be put in bottles, and kept for use.

If the composition has been boiled too much, the colour of the varnish, when mixed with the spirit of turpentine, instead of a brownish yellow, will be dark brown; and if it has not been boiled sufficiently, the varnish, when laid upon the glass, will always retain a degree of clamminess. This clamminess is likewise produced when the litharge and the saccharum saturni are not very dry; it will therefore be proper to dry those articles thoroughly



thoroughly previous to their being mixed with the oil.

This varnish is laid upon the glass by means of a hair pencil, and one coat of it is quite sufficient to preserve the insulating property of glass; but care should be had to render the glass perfectly clean and dry, and to warm it previously to the application of the varnish.

If the varnish has been rightly prepared it will dry very speedily; but for greater security it will be adviseable to leave the varnished glass in a dry place for a day or two in the summer season, and a little longer in the winter.

As some ingenious persons may be in want of the best insulating substances for nice electrical experiments, I think it not useless to mention, that raw silk possesses that property in a very eminent degree, even in damp and rainy weather. This substance, therefore, should be preferred to all others, when strength and pliability is

is required; otherwise sealing-wax and amber answer equally well.

Silks that have undergone the process of dying, or even of bleaching, though they insulate sufficiently well for common experiments, are, however, inferior to raw silk.

Quicksilver excites glass negatively.

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N° X.

*Addition to Page 35 of this Volume.*

“ I find that Mr. FONTANA observed the action of the metallic application upon the heart a considerable time before Dr. FOWLER. In a letter to Mr. MARSIGLI, dated in November, 1792, which was printed at Pavia in a periodical publication, Mr. FONTANA, expresses himself thus, “ Relativement au mouvement du  
 “ cœur, je puis assurer, qu’il est facile d’ac-  
 “ celerer ses battemens s’il est en mouve-  
 “ ment, & de le remettre en mouvement  
 “ s’il est en repos. Il suffit de le placer  
 “ entre deux métaux, par exemple, le zinc  
 8 “ & l’an-

“ & l’antimoine, de manière à ce qu’une  
 “ partie de ce muscle touche à l’un des  
 “ métaux, & l’autre partie à l’autre metal.  
 “ En faissant alors communiquer les deux  
 “ métaux au moyen d’un conducteur, on  
 “ verra s’effectuer les phénomènes que je  
 “ viens d’indiquer, meme lorsque le cœur  
 “ est séparé du corps & coupé par mor-  
 “ ceaux. Je puis encore assurer que je fais  
 “ contracter à volonté les vers de terre,  
 “ les insectes, & les animaux privés de cer-  
 “ veau & de nerfs. Sous peu de tems je  
 “ publierai un ouvrage sur le nouveau  
 “ principe du mouvement musculaire, de-  
 “ couvert à Bologne par le savant professeur  
 “ GALVANI, & j’espère demontrer d’une  
 “ maniere rigoureuse, que ce principe n’a  
 “ rien de commun avec l’électricité, & que  
 “ quel qu’il soit, il n’opère jamais la con-  
 “ traction, ni ne reproduit jamais les mouve-  
 “ mens musculaires ordinaires aux animaux.  
 “ Ainsi ce principe obscur est réduit à un  
 “ phénomène très-beau, mais dont la na-  
 “ ture & les usages restent encore à de-  
 “ terminer \*.

\* See the *Journal de Physique* for March 1793.

## N° XI.

*Addition to Page 61 of this Volume.*

“ The experiments of animal electricity are said to succeed also in the vacuum made by an air pump, viz. when the prepared animal and conducting rod are placed under the exhausted receiver, &c.—See the *Journal de Physique* for the year 1793, p. 290.

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THE  
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OF THE  
WHOLE WORK.

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